ON SOME AUSTRALIAN *ELEOTRINÆ.*

PART 2.

BY J. DOUGLAS OGILBY.

At the last meeting of the session of 1896 I had the honour to communicate to the Society a paper on the above subject, which has, I am happy to say, met with cordial approval among the most advanced ichthyologists of the colonies and elsewhere, and I take this opportunity of thanking those friends whose kind and complimentary letters encourage me in the face of many and grave difficulties to proceed with the task of bringing Australian ichthyology more into line with modern thought. It is, therefore, with great pleasure that I now, just a year later, present to your notice a second paper dealing with other members of the same interesting subfamily.

In the former paper five species were described, namely:—*Carassiops longi*, *Krefftius australis*, *Mulgoa coxii*, *Ophiorrhinus grandiceps*, and *O. nudiceps*, while the present contains diagnoses of five others:—*Carassiops guentheri*, *C. galii*, *Krefftius adspersus*, *Eleotris fuscus*, and *Ophiorrhinus angustifrons*, two of which are described as new to science, while the fauna of New South Wales is enriched by no less than three.

Owing to the magnificent series of *Eleotris fuscus*, which has lately passed through my hands, thanks to the exertions of Mr. Charles Hedley, I am enabled to present to my readers a diagnosis of the restricted genus *Eleotris*, which has been made by some authors the refuge for so many and so varied forms that it is safe to say that in no other branch of biological science would such an extraordinary agglomeration of distinct forms been permitted for so long a time. A review of the family in accordance with modern requirements, and in a publication which is likely to be accessible to all working ichthyologists, is a much needed desideratum.

The genus *Carassiops** may be amended and conveniently subdivided as follows:—

Snout scaly; dorsal spines six; caudal peduncle long and slender; vertebræ 25 ... ...
CAULICHTHYS;† type *guentheri*.

Snout naked; dorsal spines six; caudal peduncle short and deep; vertebræ 25 ... ...
CARASSIOPS;‡ type *compressus*.

Snout naked; dorsal spines seven or eight; caudal peduncle long and slender; vertebræ 30-31 ... ...
AUSTROGOBIO; type *galii*.

Appended is a list of the species which appear to belong to this genus:—

CARASSIOPS.

1. *compressus*, Krefft, Proc. Zool. Soc. London, 1864, p. 184, Clarence River.
2. *brevirostris*, Steindachner, Sitzb. Ak. Wien, lvi. i. 1867, p. 314, Cape York.
3. *reticulatus*, Klunzinger, Sitzb. Ak. Wien, lxxx. i. 1880, p. 385, Port Darwin.
4. *elevatus*, Macleay, Proc. Linn. Soc. N.S. Wales, v. 1881, p. 622, Port Darwin.
5. *longi*, Ogilby, Proc. Linn. Soc. N.S. Wales, xxi. 1897, p. 733, George's River.
6. *cyanostigma*, Bleeker, Kokos, iv. p. 452, 1855, Kokos.

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* *Carassiops*, Ogilby, Proc. Linn. Soc. N.S. Wales, xxi. 1897, p. 732.

† καυλός, a stalk or peduncle; ἴχθυς, a fish.

‡ *Auster*, south; *Gobio*, a genus of cyprinoid fishes, of which the European Gudgeon (*G. gobio*) is the type.

CAULICHTHYS.

7. *cyprinoides*, Cuvier & Valenciennes, Hist. Nat. Poiss. xii. p. 248, 1837, Bourbon.

8. *tænionotopterus*, Bleeker, Bali, p. 298, 1849.

9. *leuciscus*, Bleeker, Sumatra ii. p. 278, 1853, Western Sumatra.

10. *guentheri*, Bleeker, Versl. en Med. xi. 1876.

11. *cyprinoides*, Klunzinger, Sitzb. Ak. Wien, lxxx. i. 1880, p. 384, Murray River.

AUSTROGOBIO.

12. *galii*, Ogilby, *antea.*

No less than eight of these species or supposed species belong to the Australian fauna, and I hope soon to be in a position to give a full description of the forms not hitherto noticed.

Subgenus CAULICHTHYS.

CARASSIOPS GUENTHERI.

? *Eleotris cyprinoides*, Cuvier & Valenciennes, Hist. Nat. Poiss. xii. p. 241, 1837, Bourbon; Klunzinger, Arch. f. Nat. 1872, p. 31 *and* Sitzb. Ak. Wien, lxxx. i. 1880, p. 384, pl. v. f. 2, Murray River.

Eleotris cyprinoides (not Cuvier & Valenciennes), Günther, Catal. Fish. iii. p. 118, 1862, Sumatra & Oualan; Macleay, Proc. Linn. Soc. N.S. Wales, ix. 1884, p. 33 (copied from Günther on the authority of Klunzinger's record).

Asterropteryx guentheri, Bleeker, Versl. en Med. xi. 1876.

Eleotris guentheri, Günther, Fisch. Sudsee, ii. p. 186, pl. xiii. f. A. 1876.

Black-banded Carp-Gudgeon.

D. vi, i 8. A. i 10. Sc. 26-28/8.

Depth of body $4\frac{1}{5}$ to $4\frac{2}{5}$, length of head 4 to $4\frac{1}{5}$ in the total ngth; width of head $1\frac{3}{4}$ to 2, of interorbital region 3 to $3\frac{1}{4}$, diameter of eye $3\frac{2}{3}$ to 4 in the length of the head; snout short, its width between the posterior nostrils equal to or a little more than its length, which is as long as or a little shorter than the diameter of the eye. Maxillary not nearly extending to the vertical from the anterior border of the eye, its length from the tip of the snout $4\frac{1}{5}$ to $4\frac{1}{3}$ in that of the head. 12 or 13 gill-rakers on the lower branch of the anterior arch. The origin of the first dorsal is midway between the base of the last soft ray and the extremity of the snout or a little nearer to the former; the space between the origin of the second dorsal and the base of the caudal $1\frac{1}{4}$ to $1\frac{1}{3}$ in the remaining length; second dorsal higher than the spinous, its longest ray $1\frac{1}{4}$ to $1\frac{1}{2}$ in the head: ventral subequal to the head, the fourth ray produced, extending to or beyond the vent: pectoral with 13 or 14 rays, about as long as the ventral: caudal moderate, $3\frac{2}{3}$ to 4 in the total length; peduncle long, its depth $2\frac{1}{5}$ to $2\frac{3}{5}$ in its length, which is $\frac{1}{10}$ to $\frac{1}{3}$ longer than the head. Head-scales extending forwards on the interorbital region and snout; preorbital naked and somewhat swollen; breast-scales not much smaller than those of the body. Vertebræ 14 + 11.

Pale yellowish with a more or less conspicuous black band, one scale in width, extending from behind the upper half of the base of the pectoral to the base of the caudal, mostly below the median line; rarely this band is absent, while the scales composing it often have a lighter centre; usually all the scales above the band are dark-edged; interorbital region, snout, and a large blotch on the opercles bluish-black; mandibles and cheeks with dark dots; intermandibular region and lower lip dusky: dorsals violet, dotted with black, the second often with pearly spots posteriorly in the male.

E t y m o l o g y:—Named for Albert Günther, F.R.S., &c., the celebrated ichthyologist, keeper of the Zoological Department, British Museum, and author of many valuable works on natural science.

D i s t r i b u t i o n:—Fresh waters of Oualan, Fiji, Samoa, and New Caledonia. Not having a copy of his work, I cannot say whence Bleeker's types came.

As will be seen from the synonymy given above Bleeker has placed this fish in the genus *Asterropteryx*,* from which I have found it necessary to remove it to my genus *Carassiops*,† because in *Asterropteryx* (fide, Günther, Catal. iii. p. 132) the teeth are in a single series, and there is no genital papilla. The only differences between the group to which this species belongs and the typical *Carassiops* consist of the more depressed snout, the more elongated caudal peduncle, and the increased lepidosis of the head.

There can be no doubt that this is the species described by Günther (Catal. *l.c.*) as *Eleotris cyprinoides*, nor do I think that Bleeker's correctness in separating that species from the *E. cyprinoides* of the Histoire Naturelle can be called in question. There is, however, good reason for believing that Klunzinger's South Australian fish is distinct from both, since it differs not only in having a larger number of dorsal rays, but in the possession of no less than ten transverse series of scales between the origin of the second dorsal and the anal fins. This form might be separated as *klunzingeri*. It will, however, be necessary to make a critical comparison of all the species of *Carassiops* from different localities before these points can be definitely settled.

If Klunzinger's fish is not *Carassiops guentheri* the latter has at present no status in the Australasian fauna, but a glance at its recorded distribution will show that its ultimate inclusion within our limits is a mere matter of time; there can, therefore, be no objection to describing, and so inviting attention to it in this paper.

It is worth noting that in the New Caledonian examples the number of dorsal rays is constantly less than in those examined by Bleeker and Günther.

My largest specimen measures 90 millimeters.

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* *Asterropteryx*, Rüppell, Atl. Fisch. Roth. Meer. p. 138, 1828 *(semipunctatus)*.

† *Carassiops*, Ogilby, Proc. Linn. Soc. N.S. Wales, xxi. 1897, p. 732 *(compressus)*.

Subgenus Austrogobio.

Carassiops galii, sp.nov.

Gale's Carp-Gudgeon.

D. vii-viii, i 10-12. A. i 11-14. Sc. 29-30/8.

Depth of body $3\frac{3}{5}$ to 4, length of head $3\frac{2}{3}$ to $3\frac{4}{5}$ in the total length; depth of head $1\frac{1}{2}$ to $1\frac{2}{3}$, width of head $1\frac{3}{4}$ to $1\frac{7}{8}$, of the gently convex interorbital region 4 to $4\frac{3}{4}$, diameter of eye $3\frac{1}{5}$ to $3\frac{2}{5}$ in the length of the head; snout obtuse, $\frac{1}{5}$ to $\frac{3}{10}$ of a diameter shorter than the eye. Maxillary extending to or nearly to the vertical from the anterior border of the eye, its length $3\frac{2}{5}$ to $3\frac{4}{5}$ in that of the head. 7 gill-rakers on the lower branch of the anterior arch. The space between the origin of the first dorsal and the extremity of the snout is from $\frac{1}{7}$ more to $\frac{1}{4}$ less than its distance from the base of the last soft ray; the fifth and sixth spines are the longest, $1\frac{7}{8}$ to $2\frac{2}{3}$ in the length of the head and reaching to or beyond the origin of the second dorsal; the posterior soft rays are the longest, $1\frac{1}{5}$ to $1\frac{1}{2}$ in the head: the anal fin commences below the origin of and is similar to the second dorsal: fourth ventral ray the longest, $1\frac{3}{10}$ to $1\frac{3}{5}$ in the head and reaching to or not quite to the vent: pectoral with 15 rays, reaching to the vertical from the end of the first dorsal, $1\frac{1}{4}$ to $1\frac{2}{5}$ in the head: caudal rather short, $4\frac{1}{5}$ to $4\frac{3}{4}$ in the total length; caudal peduncle rather slender, its least depth $2\frac{3}{5}$ to $3\frac{1}{5}$ in its length, which is equal to or rather more than the head. Genital papilla oblong, extending to the anal or not so far. All the scales imbricate, those of the head, nape, and throat cycloid and smaller than the body scales, which are of equal size and ciliated. Vertebræ 30 or 31 (16 + 14-15).

Pale olive-green, more or less clouded above with purplish-brown: fins hyaline, the dorsals and anal with a broad coppery marginal band: irides silvery.

I am unable to give the true habitat of this species, which is only known to me from a stone tank in the Botanical Gardens, where it was first discovered by my friend Mr. Albert Gale, who, believing it to be new, at once informed me, and together we visited the tank and, with the permission of the Director, succeeded in capturing a number of specimens. Nothing is known as to how the fish originally got into this particular tank, but doubtless the spawn was brought thither adhering to water plants of which several species, from various up-country localities, are growing in the tank. Mr. Gale, however, believes that he has caught the fish in the Turon River. It is a small form, my largest example being but 52 millimeters in length. The majority of those in Mr. Gale's aquarium are now breeding, many of the individuals which are distended with spawn being less than an inch in length, and so delicate that the large globular ova can be distinctly seen through the cuticle. The breeding season of *C. galii* is therefore different from that of *C. longi*, which spawns in the autumn. I have much pleasure in naming this species after its discoverer.

Krefftius adspersus.

Eleotris adspersa, Castelnau, Proc. Linn. Soc. N.S. Wales, iii. 1878, p. 142, Fitzroy River.

Eleotris mimus, DeVis, Proc. Linn. Soc. N.S. Wales, ix. 1884, p. 690.

Purple-spotted Gudgeon.

D. viii, i 10-12. A. i 11-12. Sc. 32-33/11.

Depth of body 4 to $4\frac{1}{5}$, length of head $3\frac{1}{10}$ to $3\frac{2}{5}$ in the total length; width of head $1\frac{7}{10}$ to $1\frac{6}{7}$, of interorbital region $3\frac{1}{2}$ to 4, diameter of eye $4\frac{1}{3}$ to $4\frac{3}{4}$ in the length of the head; snout moderate and obtuse, its width between the posterior nostrils equal to or a little less than its length, which is $\frac{1}{5}$ to $\frac{1}{4}$ longer than the diameter of the eye. Maxillary extending to or slightly beyond the vertical from the anterior border of the eye, its length from the tip of the snout $2\frac{2}{5}$ to $2\frac{7}{8}$ in that of the head. 7 gill-rakers on the lower branch of the anterior arch. The space between the origin of the first dorsal and the base of the last soft ray is as long as or a little shorter than its distance from the extremity of the snout, that between the origin of the second dorsal and the base of the caudal $1\frac{2}{5}$ in the remaining length; second dorsal higher than the spinous, its longest ray $1\frac{3}{5}$ to $1\frac{3}{4}$ in the head: ventral $1\frac{1}{2}$ to $1\frac{2}{3}$ in the head, extending to or nearly to the vent: pectoral with 15 rays, as long as or a little longer than the ventral: caudal moderate, $3\frac{1}{2}$ to $3\frac{3}{4}$ in the total length; peduncle short and deep, its depth $1\frac{3}{5}$ to $1\frac{4}{5}$ in its length, which is $1\frac{1}{3}$ in that of the head. Vertebræ 31 (14 + 17).

Reddish- or yellowish-brown, the upper surface suffused with purple; a series of large purple spots along the middle of the side, most prominent on the tail; sides of the head with three, sometimes four, oblique purplish bands: vertical fins violet-gray with patches of dusky dots, which on the anal are confined to the basal half and the posterior rays; ventrals and pectorals immaculate.

E t y m o l o g y :—adspersus, dotted.

D i s t r i b u t i o n :—Eastern Australia.

Castelnau's types came from the Fitzroy River and are probably in the Paris Museum, but there is a fine example in the University Museum from the neighbourhood of Stanthorpe, a town of Southern Queensland. In the same Museum is a specimen of *E. mimus*, without locality, sent, Mr. Masters believes, to Sir William Macleay by its describer, which has enabled me to compare the two forms and satisfy myself as to their identity, which indeed I had previously suggested *(Vol. xxi. p. 754).* I have also examined and dissected a mutilated example, which I found in a small collection of young fishes given to me by Mr. Lucas, the exact habitat of which is unknown to him, though he is sure that they came from this colony. Some years ago I received from Mr. A. G. Hamilton several fine gudgeons from creeks near Guntawang which belong, I am satisfied, to the same form; these are now in the collection of the Australian Museum. The species may, therefore, be looked upon as ranging from the Fitzroy River in the north to the Upper Shoalhaven District in the south.

The largest of the three examined measured 77 millimeters.

Eleotris.

Eleotris, Bloch & Schneider, Syst. Ichth. p. 65, 1801.

Culius, Bleeker, Nederl. Ind. Nat. Tijdschr. xi. 1856.

Body stout, compressed posteriorly, the back broad and flat. Head large, wider than deep, depressed. Mouth large and oblique, the maxillary reaching beyond the front margin of the eye; lower jaw the longer. Jaws with a band of villiform teeth, the outer and inner series enlarged and conical. Nostrils widely separated, the anterior with a raised rim. Eyes supero-lateral. A concealed spine at the angle of the preopercle. Gill-openings rather narrow, scarcely extending to below the angle of the preopercle; six branchiostegals. Two dorsal fins, the first with six flexible spines, the second with i 8-12 rays; anal with i 7-12 rays, originating behind the second dorsal; ventral inserted behind the base of the pectoral with i 5 rays; pectoral subcuneiform, with 18 or more rays, the middle ones the longest; caudal rounded, the peduncle strong and deep. Genital papilla large, sexually dissimilar. Scales small, in 40 to 70 regular series, cycloid in front, ciliated behind; head except the snout and the anterior portion of the cheeks scaly. Vertebræ 25 or 26.

Etymology:—ἠλεός, bewildered.

Type:—*Gobius pisonis*, Gmelin.

Distribution:—Tropical and subtropical parts of the Indian and Pacific Oceans, and of the Americas.

Eleotris fuscus.

Pœcilia fusca, Bloch & Schneider, Syst. Ichth. p. 453, 1801.

Eleotris nigra, Quoy & Gaimard, Voy. Uranie, Zool. p. 259, pl. lx. f. 2, 1824.

Eleotris fusca, Günther, Catal. Fish. iii. p. 125, 1861, *and* Fisch. Sudsee, ii. p. 188, 1876, *and* Ann. & Mag. Nat. Hist. (3) xx. 1867, p. 62, *and* Voy. Challenger, Shore Fish. pp. 35, 58, 60, 1880; Macleay, Proc. Linn. Soc. N.S. Wales, v. 1881, p. 623.

D. vi, i 8. A. i 8. Sc. 62-68/17.

Depth of body $4\frac{2}{5}$ to 5, length of head $2\frac{7}{8}$ to $3\frac{1}{10}$ in the total length; width of head $1\frac{1}{4}$ to $1\frac{1}{3}$, of interorbital region $3\frac{1}{4}$ to $3\frac{3}{4}$, diameter of eye $4\frac{4}{5}$ to $5\frac{2}{3}$ in the length of the head; snout short and very obtuse, its width between the posterior nostrils subequal to its length, which is $\frac{2}{5}$ to $\frac{1}{2}$ longer than the eye. Maxillary extending to or not quite to the vertical from the middle of the eye, its length from the tip of the snout $2\frac{2}{5}$ to $2\frac{2}{3}$ in that of the head. 9 gill-rakers on the lower branch of the anterior arch. The space between the origin of the first dorsal and the base of the last soft ray is $1\frac{1}{4}$ to $1\frac{1}{3}$ in its distance from the extremity of the snout, that between the origin of the second dorsal and the base of the caudal $1\frac{1}{2}$ to $1\frac{2}{5}$ in the remaining length; soft dorsal higher than the spinous, its longest ray $1\frac{3}{5}$ to $1\frac{7}{8}$ in the head: ventral obtusely pointed, not quite extending to the vent, $1\frac{1}{2}$ to $1\frac{4}{5}$ in the head: pectoral with 17 to 19 rays, much longer than the ventral, $1\frac{1}{5}$ to $1\frac{1}{3}$ in the head: caudal large, $3\frac{2}{5}$ to $3\frac{2}{3}$ in the total length; depth of peduncle $1\frac{2}{3}$ to $1\frac{9}{10}$ in its length, which is $1\frac{1}{3}$ to $1\frac{1}{2}$ in that of the head. Vertebræ 11 + 14.

Back and sides dark olivaceous-brown or bluish-black, each of the scales sometimes with a darker central spot, forming together narrow bands; lower surfaces pale brown or bluish-white dotted with brown, the lighter colour sometimes extending in patches on the sides: fins hyaline, the dorsal and anal usually prettily marbled with black or brown, or with more or less regular series of blackish or brown spots; caudal with the basal third similar to the sides, which is sometimes preceded by a lighter band, the remainder pale brown or violet, with indistinct transverse bands; pectorals and ventrals with or without dusky spots or bands.

Etymology:—*fuscus*, brown.

Distribution:—From Madagascar through the seas of India and Australia to the Islands of the South Pacific (Solomon Islands; New Hebrides; New Caledonia; Sandwich, Fiji, Society, and Navigators Islands, Oualan).

I have never seen an Australian example, but a fine series collected by Hedley in New Caledonia has enabled me to draw up the above description.

OPHIORRHINUS ANGUSTIFRONS, sp.nov.

D. vii, i 10. A. i 10. Sc. 44/13 ca.

Depth of body $5\frac{3}{5}$, length of head $3\frac{1}{5}$ in the total length; width of head $2\frac{1}{5}$, of interorbital region 8, diameter of eye 4 in the length of the head; snout moderate and obtusely pointed, its width between the posterior nostrils less than its length, which is $\frac{1}{8}$ of a diameter longer than the eye. Maxillary extending to the vertical from the anterior border of the pupil, its length from the tip of the snout $2\frac{1}{3}$ in that of the head. 13 gill-rakers on the lower branch of the anterior arch. The space between the origin of the first dorsal and the base of the last soft ray is $1\frac{1}{5}$ in its distance from the extremity of the snout, that between the origin of the second dorsal and the base of the caudal $1\frac{1}{2}$ in the remaining length; soft dorsal higher than the spinous, its longest ray $1\frac{1}{2}$ in the head: ventral pointed, extending to the vent, $1\frac{1}{3}$ in the head: pectoral with 18 rays, subequal in length to the ventral: caudal moderate, $4\frac{1}{4}$ in the total length; depth of peduncle $2\frac{2}{3}$ in its length, which is $1\frac{1}{6}$ in that of the head. Occipital scales extending forwards to the interorbital region.

Pale yellowish-grey, the back and sides with irregular dusky blotches caused by the aggregation of minute brown dots; extremity of first dorsal dusky; second dorsal and caudal with irregular dusky bands.

Etymology:—*angustus*, narrow; *frons*, forehead: alluding to the narrowness of the interorbital region as compared with that of its congeners.

Type:—In my own collection.

Distribution:—Described from three small specimens taken in a net on Towree Point, Botany Bay, in pure salt water. The largest measured 60 millimeters.

Compared with an example of *Ophiorrhinus grandiceps* of the same size, the narrowness of the head and especially of the interorbital region is at once noticeable, as also is the greater concavity of the cephalic profile; also the ventral fins are elongate and filamentous even in the fry, while in the adult male of *O. grandiceps* they are short, even in the breeding season.

EXPLANATION OF PLATES XXIX.-XXXXIII. *bis.*

(See p. 720.)

Plate XXIX.

E. hæmastoma.

Fig. 1.—Sucker leaf (*a*), mature leaf (*b*), bud and fruit of typical form (Sydney Coast District).

Fig. 2.—Fruit (Berowra, Hawkesbury District).

Fig. 3.—Bud and fruit (National Park, 20 miles S. of Sydney). Note the graduation in size of the above fruits.

Fig. 4.—Nearly hemispherical fruit (Parramatta).

Fig. 5.—Flat-topped, somewhat oblique, pear-shaped fruit (Peat's Ferry, Hawkesbury).

Plate XXX.

E. hæmastoma.

Fig. 6.—Variety with fruits in a head; also a mature leaf, which much resembles the sucker foliage of the normal form (Mt. Victoria).

E. hæmastoma (micrantha).

Fig. 7.—Sucker and mature foliage and fruits (Mittagong District; on ridges).

Plate XXXI.

E. hæmastoma (micrantha).

Fig. 8.—Mature foliage and fruits (Mittagong District; on flats).

Fig. 9.—Fruit (Mt. Victoria).

Fig. 10.—Small mature leaf (Sydney District).

Fig. 11.—Portion of an umbel, Cabbage Gum of the North Coast Districts. There are usually 8 or more fruits in an umbel.

Fig. 12.—Tazza-shaped fruits from the South Coast.

Fig. 13.—Umbel of fruits from Grenfell District, showing thin, long pedicels and flat-topped sharp-rimmed fruits. Sometimes there are 20 in a head.

Fig. 14—Mature leaf and umbel from Rylstone District.

E. stricta.

Fig. 15.—Mature leaf and fruit. Note the urceolate shape of the fruit (Mt. Victoria).

Fig. 16.—Fruit showing oblique shape (Mt. Victoria).

Fig. 17.—Leaf and fruit. The tips of the valves are flush with the top of the fruit (Lawson, Blue Mountains).

Fig. 18.—Fruit and leaf of var. *rigida* (Wentworth Falls).

Plate XXXII.

E. Luehmanniana.

Fig. 19.—(*a*) Sucker leaf.

Plate XXXIII.

E. Luehmanniana.

Fig. 19.—(*b-c*) Mature leaves of ordinary size. (*d*) Buds, showing flattened peduncle. (*e*) Fruit, showing corrugated surface, also the broad, flattened rim (National Park, near Sydney).

Plate XXXIII. *bis.*

E. Luehmanniana, var. *altior*.

Fig. 20.—(*a*) Mature leaf. (*b*) Pointed buds, with flattened peduncle. (*c*) Fruit, showing flattened, broad rim (from Mt. Wilson).

E. obtusiflora.

Fig. 21.—(*a*) Sucker leaf. (*b*) Mature leaf. (*c*) Clavate buds. (*d*) Fruit (National Park).

Fig. 22.—(*a*) Mature leaf. (*b*) Bud, showing pointed operculum. (*c*) Fruit, slightly domed. This is the form (γ) from the Spit, Port Jackson, referred to in the text (p. 715) as a connecting link with *E. Luehmanniana.* Note the transverse veins starting out at a fairly uniform angle to the midrib.

Fig. 23.—Subcylindrical fruit.

Fig. 24.—Flat-topped fruit; in shape not unlike that of *E. stricta*, but with a thicker rim.

ON THE EVIDENCE (SO-CALLED) OF GLACIER ACTION ON MOUNT KOSCIUSKO PLATEAU.

By the Rev. J. Milne Curran, Lecturer in Geology, Technical College.

(Plates xxxvii.-xxxix.)

[Read in abstract November 25th, 1896 (P.L.S.N.S.W. 1896, p. 819); but publication deferred to allow of the author's again visiting Mount Kosciusko.]

In January, 1885, Dr. R. von Lendenfeld made a visit to Mount Kosciusko. Shortly afterwards he issued a Report* dated 21st of January, 1885, and addressed to the Minister for Mines, in which he states that he found "rocks polished by Glacial Action"† in many places. Sometime afterwards he published a paper entitled "The Glacial Period in Australia."‡ Dr. Lendenfeld comes to the conclusion that Glaciers extended from a high Plateau, Mount Kosciusko—down into the valleys around; he noted that in these valleys "most beautiful and indubitable traces of glacial action";§ that evidences of Glaciation were "found in the shape of *Roches Moutonnées* scattered over an area of one hundred square miles."|| There can be no doubt Dr. Lendenfeld is referring to a Post-Tertiary Glaciation, for he adds, "that portion of Australia was, therefore, not so long ago, certainly covered with ice."¶ More recently Mr. Richard Helms accepted

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* Report by Dr. R. von Lendenfeld on the results of his recent examination of the central part of the Australian Alps. Sydney. Thos. Richards, Government Printer. 1885.

† Dr. Lendenfeld's Report, p. 10.

‡ Proc. Linn. Soc. N.S.W. (1st Series), Vol. x. p. 48.

§ *Loc. cit.*, p. 47.

|| *Loc. cit.*, p. 50.

¶ *Loc. cit.*, p. 50.

these conclusions and contributed a paper* to this Society embodying some fresh observations. Mr. Helms concludes that there are "evidences of extensive glacier action at Mount Kosciusko," and that "many of the rounded, concave, and level surfaces found upon a number of the large rock facings have been produced by glacier action, although the minute features of it have long since been destroyed by erosion and decomposition."†

The present writer spent three weeks on the Kosciusko Plateau since the publication of the papers referred to. On my first trip I was accompanied by Mr. Charles Hedley, of the Australian Museum, and Mr. James Petrie, of the University. The route traversed is shown on the accompanying map. This record of the routes taken I consider of some importance, as the first essential for the forming of an opinion on the physical features of a locality is to actually go over the ground. I confess I went to Mount Kosciusko fully prepared to see the evidences of glaciation as observed by the authors referred to. Mr. Helms regrets "that time did not permit to make closer observations," and speaks of a lake "which like all the other features received only a passing glance."‡ I have no desire to dictate conditions to other writers, but when important conclusions are voluntarily placed on record, in the pages of a scientific publication, I think the authors should hardly plead want of time as an excuse for hasty observation. Dr. Lendenfeld certainly does not complain of want of time, but I am aware that he did not spend more than a few days on Mt. Kosciusko, so that his observations must have been of the nature of a general reconnaissance rather than a detailed examination. To guard against hasty conclusions, I undertook a second journey to Mt. Kosciusko, and, with the conditions of excellent weather and ample time, I made the observations embodied in this paper.

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* Proc. Linn. Soc. N.S.W. (2nd Series), Vol. viii. p. 349.

† *Loc. cit.*, p. 352.

‡ *Loc. cit.*, p. 364.

Before dealing with my own observations, it is convenient to note that Dr. Lendenfeld found the strongest evidences of glaciation in the Wilkinson Valley. Now, Mr. Helms admits that wherever else he saw evidences of glaciation, he certainly saw none in the Wilkinson Valley, and just in the same way as Mr. Helms could see no evidence of glaciation, where Dr. Lendenfeld found such evidences to abound, I confess I have failed to see evidences of glaciation in any one of the localities indicated by Mr. Helms. Mr. Helms issued a map with his paper, on which map he coloured certain places (in blue) where what he terms "glacier traces" are specially pronounced. He also marks certain limited areas (in black) which he calls snow fields. From my standpoint these glacial traces have no existence, and as for the snow fields, I am able to say that on 20th January, 1896, there was not a square yard of snow on any part of the Kosciusko Plateau. I cannot, therefore, agree with Mr. Helms' opinion that "they never entirely disappear even in the hottest summers, and it may safely be said that they remain permanent over the limited area."

I leisurely examined every tract of country coloured blue on Mr. Helms' map; and, taking that map as a basis, I will deal with the so-called glacial traces, beginning with those immediately under Mt. Kosciusko. There is one thing to be noted about this map that has caused a good deal of confusion: Mr. Helms (following Dr. Lendenfeld) calls the highest peak Mount Townsend, and I have satisfied myself that he was not justified in so doing. To begin with, Dr. Lendenfeld ascended a mountain which *his guides told him was Mt. Kosciusko.** He discovered another peak

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* There was no other means of judging. No accurate maps were available at the time of Dr. Lendenfeld's visit. All that is really known is that Strzelecki named the highest peak, or what he took to be the highest peak, Mt. Kosciusko. Dr. Lendenfeld assumes too readily that Strzelecki did not ascend the highest point, although the distinguished Polish traveller had quite as many facilities for observation as had Dr. Lendenfeld. It must be remembered also that the two peaks, Mts. Townsend and Kosciusko, are within an easy walk of each other.

a mile to the south, which he found to be higher than the peak pointed out to him as Mt. Kosciusko. Thereupon he names this highest point Mt. Townsend. One of the residents of Monaro, Mr. John Barry, assured me that tourists were usually taken to a peak which he was well aware was not Mt. Kosciusko. Dr. Lendenfeld was misled in this way. When speaking of Mt. Kosciusko in this paper, it will be understood that I refer to the peak due south of Lake Albina, and called by Dr. Lendenfeld Mt. Townsend. There is another point about Mr. Helms' map. He speaks of various mountains on the Plateau as Mt. Etheridge, Mt. David, Mt. Tenison-Woods, Mt. Townsend, etc. This I think most undesirable. The whole mountain, as well as the highest point, should be called Mt. Kosciusko, and other eminences of note might be called peaks. It would be more satisfactory to speak of the Etheridge Peak, the David Peak, etc., and *Mount* Kosciusko.

Coming now to the evidences of glaciation, I first examined the valley of the Crackenback River. Dr. Lendenfeld is very definite in stating that " there was a small glacier at the head of the Crackenback."* Dr. Lendenfeld has not stated that he examined the country at the head of the Crackenback. I have reason to believe that he was never there. But as the statement is so very definite, I expected to find some evidence that the glacier once existed. In order to examine the country thoroughly, I left the beaten track and crossed over from Moonbar to the Mowambar or Moonbar River, and followed that stream by easy stages to its very source. I then crossed the divide to the head of the Crackenback. The rocks consisted entirely of granite in many varieties. No volcanic rocks or dyke rocks were observed. A portion of the valley was extensively turned over in times past by alluvial miners in search of gold. Around the old shafts the alluvial deposits may still be seen, consisting of boulders, shingle, and pebbles of granite, showing little or no signs of

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* Proc. Linn. Soc. N.S.W. (1st Series), Vol. x. p 53.

decomposition. Nowhere in the valley of the Crackenback or at its head could I find any traces of grooved or scratched pebbles, or any features that would suggest *Roches Moutonnées*. Neither could I find any trace of Moraines. Very often masses of boulders might be noted, evidently transported from higher ground; but neither the boulders, nor the detrital masses of which they form a part, gave the least indication of glacial action. Undoubtedly, as Mr. Helms puts it, "rocks showing rounded, concave, and level surfaces"* are abundant. But most certainly none of these features can without strong collateral evidence be attributed to glacial action. From the Crackenback Valley I travelled along the main range to the foot of Mt. Kosciusko. Naturally I turned to the Wilkinson Valley for some of the evidence that Dr. Lendenfeld found so abundant. I camped here for a week, but long before that time elapsed I was forced to the conclusion that Dr. Lendenfeld was utterly mistaken in attributing any of the features in the Wilkinson Valley to glacial action. Thus far Mr. Helms agrees with me. Dr. Lendenfeld is very definite in his statement that he "found glacier-polished rocks in several places."† Mr. Helms could see none of these polished surfaces in the Wilkinson Valley. Let me add that I could see none of them either. In despair at finding any of the traces that were so evident to Dr. Lendenfeld, I decided to visit other places indicated on Mr. Helms' map as affording the "glacier traces." This map appears as Pl. XVIII. in Proceedings of this Society (2nd Series), Vol. viii. Map in hand I journeyed to Lake Albina, on which lake Mr. Helms shows a peninsula jutting into the lake, and he colours this peninsula blue, as affording evidence of "glacier traces." Here is a photograph showing the lake and the peninsula. (Pl. xxxvii., fig. 1). In the picture there is nothing to be seen suggestive of ice action. On examining the place itself there is absolutely nothing to be found indicative of ice-action. There is in fact no feature about the lake, the cliff, or the talus at its base, that may

* Proc. Linn. Soc. N.S.W. (2nd Series), Vol. viii. p. 352.

† Proc. Linn. Soc. N.S.W. (1st Series), Vol. x. p. 47.

not be amply accounted for by forces actually in operation. Indeed, the more closely I examined the talus at the base of the cliff beyond the lake, the more astonishing it seemed that any feature observable could, in the most distant way, suggest ice action. Indeed, I will go further and say that if the evidences in favour of glaciation on every point of the Plateau were overwhelming, if we could point to grooves, furrows, scratches, moraine deposits, and boulder masses, and if we had an abundance of *Roches Moutonnées*, I would still make exception of the shores of Lake Albina, and conclude that there, at any rate, no traces of glacial action were in evidence, and nothing suggestive of ice action was preserved. I lingered a day longer in this locality in the hope that any evidence however slight might be forthcoming in favour of the position taken up by Dr. Lendenfeld and Mr. Helms. Nothing more was discovered, and, therefore, I place it on record that in my opinion there is nothing to the eye of the geologist indicative of ice action on the shores of Lake Albina. Turning again to Mr. Helms' map we find that there are tracts coloured blue, in a line directly under Lake Albina; in other words, in a direct line south-east of the lake. To prevent any confusion, it may be noted that one of these blue patches covers the word "dividing," and the other is situated on the Snowy River, between its source and the junction of its first affluent on the right bank. I took special pains to locate these two areas, and in fact examined every square yard of the ground. Once again I was forced to conclude that Mr. Helms has misinterpreted the facts observable; I could not find anything whatever of his "glacier traces." There is abundance of what Mr. Helms calls rock débris. "We could observe," remarks Mr. Helms, "extensive flats with large rocks sticking out of the surface here and there, and bogs all over them"; but I am utterly unable to see what grounds there are for Mr. Helms' conclusion that "these flats have been formed by ice."* Three miles to the south of the Perisher, as shown in Mr. Helms' map, two other areas may be noted, coloured blue, as

* Proc. Linn. Soc. N.S.W. (2nd Series), Vol. viii. p. 353.

showing glacier traces. There is some difficulty in determining the exact position of these areas, as the map is not quite accurate here in the contour of the hills. I am not prepared to state absolutely that I found the identical place referred to by Mr. Helms, and on that account I cannot be too positive. I cannot believe, however, that I did not actually traverse the ground, the locality not being far from the main track, but nothing at all suggestive of ice action caught my eye. As Mr. Helms does not refer particularly or definitely to these two places, I pushed on and formed a camp at Pretty Point, so as to be centrally situated in the most extensive glaciated areas shown on Mr. Helms' map. Mr. Helms is very definite in his conclusion concerning this locality, and speaks about an open grassy flat at Wilson's Valley. This valley he says "may safely be considered attributable to glacier action."* A few lines further on he says, "entering the flat we stand on Boggy Plain and upon an unmistakable glacier deposit." This I consider the most astonishing statement in Mr. Helms' paper. The assertion simply bewilders one. I cannot conceive how such a conclusion could have been reached: to my mind this one fact is abundantly, unmistakably clear—Boggy Plain is not a "glacier deposit." There is nothing that one can appeal to, nothing that one can point to, indicative of ice action. "Proceeding," says Mr. Helms, "the evidence of ice action is becoming more plentiful at almost every turn." I have to state simply that I saw nothing of the sort. This was not attributable to any want of care or observation on my part. I went to the Kosciusko Plateau believing that evidences of glaciation were abundant, and it was with the utmost reluctance that I was forced to come to the conclusions here recorded.

The value of Mr. Helms' observations could be tested critically on Boggy Plain. Nowhere is he so definite in his statements as when speaking of Boggy Plain. I decided on this account to examine the plain thoroughly, and nothing could be easier than

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* Proc. Linn. Soc. N.S.W. (2nd Series), Vol. viii. p. 354.

such an examination. The season was a fairly dry one, and a number of shafts had been sunk some years ago, during a particularly dry season, at various points on the plain in search of gold. These shafts were put down in exactly the way a geologist would like to have them, namely, in the "deepest ground," as it was the miners' desire to get through the drifts on to bed rock, in their search for gold. I was agreeably surprised to find in these shafts boulders of pure quartz, quartz-porphyry, and diorite, the two last-named rocks being for the most part perfectly sound, and showing very little signs of decomposition. I exhibit some of the boulders collected by me. They are just of the right material and in the proper state of preservation to show any traces of grooving or scratching—if grooved or scratched they ever had been. Take this boulder of diorite, for instance, the very finest scratches would be preserved here, had they ever been made. I examined hundreds of stones of this sort out of the shafts from positions where a geologist would have selected them, had the shafts been sunk for his own particular use, but never once did I find a grooved boulder, or striated pebble, or a polished surface. The stones in these shafts are not angular, but, on the contrary, well water-worn and rounded. Mr. Helms points out that polished surfaces are not to be expected, nor grooves nor striæ to be looked for on the gneissic granite and slate rocks, as he observes "they would not retain polish or striation for any length of time." Indeed, Mr. Helms' paper would lead one to believe that slate and gneissic granite were the only rocks on the plateau. I would point out that there is basalt a short distance from the top of Mt. Kosciusko. There is basalt also a little to the north of Mt. Townsend. There is a picrite-basalt at Lake Merewether, and quartz-porphyry and diorites must be abundant from the quantities of boulders of these rocks found in the shafts. Let me insist on the fact that *all* the boulders in the shafts on Boggy Plain are water-worn; even the blocks of quartz are rounded. If these water-worn stones are the work of a glacier, I can only say that every alluvial gold-field in New South Wales is rich in "glacial traces"—a somewhat absurd, but necessary conclusion.

Abandoning all hope of here finding support for Dr. Lendenfeld's "Glacial Period in Australia," I turned to the valley named on Mr. Helms' map Glacier Valley. There is little to be gained by describing this valley and its rocks in detail. Nothing that I saw altered the opinion already expressed. Rounded rocks there are, and smoothed rocks also, with contours that probably *could* be produced by ice, but on a critical examination even that probability vanishes.

There remains but one other tract on Mr. Helms' map to deal with. This is the area around Lake Merewether, named Evidence Valley, I presume on the "lucus a non lucendo" principle. There are many features in this tract that may require or suppose the existence of ice-sheets and snow-fields, and the violent action of heavy streams of water flowing under ice; but there is nothing to warrant one's supposing the existence in the past of *moving* ice.

Some of the individuals of the large rock masses in this valley are strikingly angular. A photograph (Pl. xxxviii., fig. 2) will show the actual breaking up of granite into rectangular blocks by natural weathering. Many of these blocks are as rectangular as if hand-dressed from a quarry. The vast piles of blocks, many of them of this description, between the Hedley Tarn and the Snowy River, are a somewhat exceptional occurrence. I should not, however, feel justified in supposing *moving* ice to have brought these massive rocks together. Other collateral evidence ought to be forthcoming of the existence of glaciers. I mean by collateral evidence, such evidence as is afforded by scratches, grooves, and furrows on rocks, boulder clays, angular blocks, *Roches Moutonnées*, perched blocks, transported blocks, moraines and moraine deposits.

In accounting for the origin of masses of boulders in such a region as that we are dealing with, it may be well to bear in mind that the forces of disintegration and decomposition are far more intense in their action here than under normal conditions. We should remember, too, that we are dealing with possibly one of the oldest land surfaces on the globe. The destruction by weathering, including in that term disintegration and decomposition, must be enormous on mountain peaks like Mt. Kosciusko. Many of the great packs of loose rock material have no doubt been formed by selective process, the smaller stones being carried away where heavier masses remain. I have noticed immense blocks undoubtedly carried a short way down some valleys—blocks which one could hardly suppose were carried by running water. I can see no reason, however, for assuming that ice was the transporting agent. By a continuous undermining (by running water) of the softer materials on which they rested, they could easily have been moved into their present positions.

Some of the granite rocks to the south and west of Lake Merewether have a decidedly rounded and smoothed appearance, but not more pronounced, I should say, than that familiar to every geologist in the granite districts of New England and even in the neighbourhood of Bathurst and Cowra.

I repeat that it may be necessary to assume the existence of thick sheets of ice to explain some of the features noted, and we may even utilise valleys filled with snow, over the frozen surfaces of which boulders may have slid; but assuredly there is no feature in Evidence Valley that requires moving ice to explain it.

Dr. Lendenfeld and Mr. Helms have assumed throughout that there is above their supposed glaciers a gathering ground where snow could accumulate and consolidate into ice, and so form a feeding ground for the glaciers. A few hundred yards from the great glacier, supposed by Mr. Helms to have come down from Mt. Twynam, we have the very summit of a sharp divide, with a rapid fall away on the other side. We have, in fact, a glacier without a gathering ground, a condition of things not easy to understand. Dr. Lendenfeld in like manner fills the Wilkinson Valley with a glacier. The learned doctor from his experience very well knew that a glacier must have a gathering ground. Following up the Wilkinson Valley from the point where Dr. Lendenfeld makes his glacier do most of its work we come, in about half a mile, to the summit of the divide, from which point another valley dips away on the opposite side. It is reasonable to ask: where were the snow-fields and the gathering ground for the glacier of the Wilkinson Valley? Dr. Lendenfeld replies by assuming their existence, and from my standpoint as a geologist I protest against this assumption on his part. Take Dr. Lendenfeld's plate of the Wilkinson Glacier (Proc. Linn. Soc. N.S.W. Vol. x. Pl. 7), sketched from Mt. Townsend (our Mt. Kosciusko). It will be noted that away to the back of the range, showing as he says polished rocks "en face," he makes mountains rise tier above tier. Pl. xxxvii., fig. 2, is a photograph taken from approximately the same spot that Dr. Lendenfeld sketched from. It will be noted that there are no mountains rising above or beyond the range across the valley, and more than that I assert that standing on the very highest point of Kosciusko (Mt. Townsend of Lendenfeld) and looking in the direction in which the Doctor sketched, no mountains or table-lands are visible above the range across the valley. In other words, the view is bounded in that direction by the outline of the range, round the base of which Dr. Lendenfeld asserts the glacier wound. The long stretches of great mountains that appear on Dr. Lendenfeld's plate as showing above the Abbott Range, when seen from Mt. Townsend (our Kosciusko), do not exist. In a word, a serious difficulty in the case of the supposed Wilkinson glacier, and the supposed Helms glacier described as coming over Townsend, is that these glaciers have no place to come from. It may be argued that the plateau, which must be postulated in each case, has disappeared by being denuded away. Possibly, but if these great mountains and plateaux have been planed down, since the "glacial period," there is little hope for the polished rocks of Dr. Lendenfeld, or the rounded rocks of Mr. Helms, being preserved. Either supposition is fatal to the position taken up by Dr. Lendenfeld and Mr. Helms.

Before concluding I may say that, at several points on the plateau, I found polished or rather smoothed faces on rocks. In every instance this was due to slickenside. On the end of a ridge that bounds the valley of the stream that flows from the Garrard Tarn I noted a surface of several square yards of *polished* rock. The rock was a micaeous slate, and I was somewhat puzzled to account for the polish on so soft a rock. Besides, the polished surface stood nearly vertical. This of course *could* be caused by moving ice, by supposing the valley to be filled with a glacier. On examining the specimens by slicing them for the microscope, I found that the polish was due to a thin coating of silica. Now a glacier may smooth a rock and polish it, but certainly not coat it with silica. The explanation is that the point at which I collected my specimen is close to the boundary of an intrusive granite. The slate is much faulted, broken, and contorted, and the specimen referred to is part of a slickenside formed at the time of the intrusion of the granite.

So far I have not alluded to the evidences of glacial action in recent times that have been described as occurring to the South of Mt. Kosciusko and for the most part within Victorian borders. Many of the descriptions published are circumstantial in every way and cannot be lightly put aside. As I have not been over the ground I cannot offer any criticisms from my own knowledge. It seems to me, however, that in most of the instances quoted the characters referred to glacial action could have been as well attributed to other causes. In a word, if overwhelming evidence was forthcoming as to the glaciers described by Dr. Lendenfeld having existed in fact, then features that could have been produced in another way might safely be attributed to glacial action. The instances cited seem to me something in the nature of collateral evidence depending entirely for its value on the *fact* of a glacial period. Mr. R. M. Johnston* has summarised the papers referred to in a manner which leaves no doubt of the great weight of evidence that has gradually accumulated in favour of recent glacial action. I refer to this evidence merely to point out that it cannot be ignored, and to emphasize the fact that this paper deals only with Mt. Kosciusko and the country immediately round: a tract that may be defined as embraced in the map published herewith as well as that published by Mr. R. Helms.†

My conclusions may be summed up as follows:—I have been over the same ground as Dr. Lendenfeld and Mr. Helms. I

* Papers and Proceedings of the Royal Soc. of Tasmania for 1893, p. 73.
† Proc. Linn. Soc. of N.S.W. (2nd Series), Vol. viii. p. 349.

could not but agree with Mr. Helms as to the absence of any evidence of glaciation such as Dr. Lendenfeld had reported in Wilkinson Valley. But I also feel compelled to differ from Mr. Helms in respect of the other localities in which he believed he had detected evidence of "glacier action," as indicated on the map accompanying his paper; and I am forced to the conclusion that the evidence adduced is wholly insufficient, and that no striæ, groovings or polished faces (due to ice-action), or *roches moutonnées* perched blocks, moraine-stuff, or erratics are to be met with. Only one example of anything like a polished block was noted, and in this case the polishing and striæ-like markings were clearly due to a "slickenside." Most of the granite is of a gneissic character, but normal granites are also present, the latter weathering into spheroidal masses of disintegration, the contours of which in a few cases are suggestive of ice action. There is no collateral evidence to support any such suggestion. It has been stated that the rocks on the plateau are not such as would preserve glacial striæ. This is not strictly in keeping with fact, as I found porphyries, diorites and basalts, as well as abundance of quartz pebbles and boulders in the drifts. Apart from local evidence, the general contour of the valleys is not in the least suggestive of glaciers. I therefore concluded that (1) there is no satisfactory evidence of glaciers having once filled the present valleys; (2) there is absolutely no evidence of extensive glaciation on the Kosciusko Plateau; (3) The glacial epoch of Australia in Post-Tertiary times, as described by Dr. Lendenfeld, has no foundation in fact. Neither are there any snow-fields with "eternal snow," however limited, on Mt. Kosciusko.

Postscript.—After this paper was written a paper embodying an extended series of observations on evidences of glacial action on the Australian Alps across the Victorian border was read to the Sydney Meeting of the Australasian Association for the Advancement of Science by Messrs. Kitson and Thom. It seems to the present writer that the case made out by these authors in favour of recent glacial action in the Australian Alps is no stronger than that of Dr. Lendenfeld and Mr. R. Helms.

EXPLANATION OF PLATES.

Plate XXXVII.

Fig. 1.—Lake Albina (Mt. Kosciusko in the distance to the left). This picture is taken from a ridge North from Mt. Kosciusko, and the peninsula jutting into the lake is that distinctly marked on Mr. R. Helms' map as preserving "glacial traces." All the country shown here is above the tree-line. The gully, seen in the distance, forming a feeder to the lake, marks the exact line of junction between slate and granite country. The hills to the right are slate; Mt. Kosciusko itself and the country to the left of the gully are granite varying from typical to gneissose granite.

Fig. 2.—This photograph is taken approximately from the western shoulder of Mt. Kosciusko, and from approximately the same point as that from which Dr. Lendenfeld made his sketch shown in Vol. x., Plate 8, of the Proceedings of this Society. Mueller's Peak (Mt. Townsend) is seen in the distance to the right. Looking in the same direction from which this photograph was taken no ridges are visible above the horizon shown. This may be said even of a view taken from the very summit of Mt. Kosciusko. The country shown is all above the tree-line.

Plate XXXVIII.

Fig. 1.—The Garrard Tarn. There is no tarn or lake in Kosciusko that affords direct evidence of ice-action. The tarn shown is, in the author's opinion, a dammed up elbow of an ancient stream; but immediately in the background a cirque or corrie may be seen in course of formation which will in time form an independent tarn or add to the area of the tarn figured.

Fig. 2.—A little to the N.E. of Lake Merewether, granite may be seen breaking up into the angular blocks shown. The sheeted structure of much of the granite on the plateau lends itself to the production of vast masses of detrital matter in which the granite boulders show parallel and plane faces. When this sheeted granite is traversed by joints, weathering gives rise to large quantities of angular blocks.

Plate XXXIX.

The Kosciusko Plateau showing the author's route and all the known lakes and tarns.

NOTES AND EXHIBITS.

[N.B.—The October and November Notes and Exhibits have inadvertently been transposed.]

Mr. Brazier, on behalf of Mrs. Kenyon, exhibited specimens of the following Mollusca (Fam. *Veneridæ*) found on the Victorian coasts, and contributed a Note on the same:—*Venus gallinula*, Lam., *V. australis*, Sowb., *V. scalarina*, Lam., *V. Peronii*, Lam., *V. aphrodina*, Lam., *V. spurca*, Sowb., and *Tapes flammiculata*, Lam., originally described under *Venus*.

Mr. Brazier exhibited, and contributed a Note descriptive of, a new Volute from the Lakes Entrance, Victoria. The only specimen available at present is unfortunately somewhat broken.

Mr. A. H. Lucas exhibited examples of extreme fasciation in the Flannel Flower *(Actinotus helianthi)* and *Calycothrix tetragona;* also specimens of *Casuarina glauca* infested with *Eriococcus turgipes*, Maskell (determined by Mr. Froggatt). Wherever the parasite had attached itself the shoot had made a simple bend round it, growth of the shoot being hindered on the side next the scale. This arrangement gives a simple means of covering and protection to the parasite. A whole avenue of the Casuarina was so affected.

Mr. Froggatt exhibited a number of scale insects *(Eriococcus coriaceus*, Mask.), upon a twig of Eucalyptus, among which had been placed a great number of the eggs of the scale-eating moth *Thalpochares coccophaga*, Meyr. The eggs are pale pink, circular, and beautifully ribbed. The scales were infested with the larvæ of *Cryptolæmus montrouzieri*, Muls., one of the useful small black ladybird beetles. Both these enemies of *Eriococcus* are of great economic value, as the moth larvæ have now taken to eating the olive scale *(Lecanium oleæ*, Sign.); and the ladybird beetle is systematically bred both in New Zealand and America. Also living specimens of the largest Australian white ant, *Calotermes longiceps*, Froggatt, which were taken out of a log of fire-wood, and had already been in captivity for over two months.

Mr. W. Forsyth, on behalf of Mr. Maiden, exhibited flowering specimens of three rare plants, *Phebalium elatius*, Benth., from the Mongani Mountain, District of Gloucester, N.S.W.; *Pherosphæra Fitzgeraldi*, F.v.M., from the Blue Mountains, and *Myoporum floribundum*, A. Cunn., from the Nepean River.

Mr. Ogilby exhibited the specimen of Trachypterus described in his paper.

Mr. Fletcher showed a series of Tasmanian and West Australian frogs in illustration of his paper.

WEDNESDAY, MARCH 30TH, 1898.

The Twenty-fourth Annual General Meeting of the Society was held in the Linnean Hall, Ithaca Road, Elizabeth Bay, on Wednesday evening, March 30th, 1898.

Professor J. T. Wilson, M.B., Ch.M., President, in the Chair.

The Minutes of the previous Annual General Meeting were read and confirmed.

The President then delivered the Annual Address.

PRESIDENTIAL ADDRESS.

In reviewing the work of the past Session, it is satisfactory to be able to report that it has been a decidedly busy one. The full number of Meetings was held, the number of papers read being forty-nine. The majority of these are contained in the three Parts of the Proceedings for 1897 which have been published and distributed. The balance of the papers are already in type, so that the concluding Part is well advanced.

Nine Ordinary Members were elected into the Society during the year; one Member resigned on his departure from Australia; and the Roll has been further depleted by the demise of one Ordinary and one Corresponding Member.

Mr Robert Cooper Walker, late Principal Librarian of the Sydney Public Library, who died on July 25th, 1897, in his 65th year, was one of the Society's Original Members who kept up his membership to the last. He was the son of the late Rev. James Walker, M.A., Oxon., some time Head Master of the King's School, Parramatta. Mr. Walker was born in England, but came to the Colony while still young. He entered the public service in 1855; and in 1869 he was appointed to the position of

Principal Librarian of the Public Library, which he held until his retirement on a pension in 1893. The Public Library developed very considerably during Mr. Walker's lengthy administration. One branch of it especially commanded his attention, namely, the literature relating to Australasia. As a result, and with the co-operation of the Trustees, the Sydney Public Library now possesses a very fine collection of publications and documents of this character; and in 1893, under Mr. Walker's editorship, a bulky quarto bibliography relating to the same was published. Mr. Walker was not directly interested in the special pursuits which it is the primary object of this Society to foster. His membership, as in the case of many of the Original Members, was to some extent rather the expression of his sympathetic recognition of the claims of a Scientific Society for support on the broad general grounds of education and culture.

No doubt the Society was most in need of support in the critical period of its very early history. The number of those in a position to contribute papers, and the amount of work done, have since then been steadily on the increase. But, taking into account the general increase in the population and the considerable development of the Colony since 1875, the Council cannot but regret that there has been a falling off in the membership, especially in that section of it of which Mr. Walker was a representative.

Professor Thomas Jeffery Parker, D.Sc., F.R.S., who died on November 7th, 1897, at the early age of 47, was elected a Corresponding Member in 1893. He was the eldest son of the late William Kitchen Parker, F.R.S., the well-known comparative osteologist and morphologist, and was alike eminent as teacher and as investigator. Until his appointment to Otago in 1880, Professor Parker for a number of years was Professor Huxley's Demonstrator at the School of Mines, and there he materially assisted his distinguished colleague in developing the biological portion of the curriculum along the lines which made it as a biological course second to none in the United Kingdom. In "Nature" of January 6th, 1898, will be found a most interesting and appreciative sketch of Jeffery Parker's life and work, written by his old friend, colleague and successor, Professor G. B. Howes, F.R.S. It is so difficult for one possessed of less personal knowledge to speak effectively on a subject of this kind after a deliverance by one who has spoken with full personal knowledge, that it were wise not to attempt to supplement what Professor Howes has so admirably and so sympathetically given us. I may content myself, therefore, with commending to your perusal the article to which I have referred.

It is fitting, also, that some reference should be made to two other well-known members of the community who passed away in July last, within a few days of each other and of Mr. Walker. Sir Patrick Jennings, K.C.M.G., who died on July 11th, aged 66 years, was an Original Member of the Society who maintained his membership until quite recently. He was well known by his lengthy political career, by his association with the cause of higher education as a member of the Senate of the University, and by his great interest in art and music, and in public affairs generally.

The Venerable Archdeacon R. L. King, B.A., Cantab., who died on July 24th, 1897, aged 74, though never a Member of this Society, actively co-operated with Sir William Macleay in carrying on the work of the Entomological Society of New South Wales, to which he contributed a number of papers, and of which for two years he was President. Mr. King was the eldest son of the late Admiral King, so well known in the annals of Australian maritime exploration. During his residence in Parramatta as Incumbent of St. John's Church, and for some little time after his removal to Liverpool as Principal of the Moore Theological College, Mr. King took up the study of natural history as a hobby, and for a busy man he succeeded in accomplishing a surprising amount of entomological and other zoological work, until the pressure of official duties and want of leisure obliged him to give it up altogether. With the exception of several papers on Entomostraca contributed to the Royal Society of Tasmania, and published in the Papers and Proceedings for 1852-54, all Mr. King's papers will be found in the two volumes of Transactions of the Entomological Society of N. S. Wales.

In December last our respected Hon. Treasurer, the Hon. Dr. Norton, communicated to the Council his wish to be relieved of the responsibilities of office, finding it desirable in the interests of health to forego some of the official duties with which in his leisure he has long voluntarily occupied himself. In accepting Dr. Norton's resignation, the Council unanimously resolved that there should be entered on the official records a minute expressive of the Council's regret at his retirement, and of its appreciation of the valuable services which Dr. Norton had cheerfully rendered to the Society without intermission since January, 1882.

Under the new rules now in force, the appointment of the Hon Treasurer rests with the Council. I am glad to be able to report that, on the nomination of his predecessor, Mr. P. N. Trebeck. whose business qualifications are of a high order, was elected to and has kindly consented to fill the vacancy which under the gradual unfolding of the plans of the founder of the Society is not now the sinecure it used to be when the Society's finances were on a more humble scale.

An important achievement of last Session was the revision and extension of the Society's Rules. The Council had learnt by experience that some such step in this direction was to be desired. Early in the year a Committee was appointed to consider and report on the whole question. The efforts of the Committee—and in this connection special mention must be made of the valuable assistance rendered by Mr. J. R. Garland—resulted in a draft which was submitted to the Council, and after full consideration and with a few amendments adopted. It was subsequently submitted at a Special General Meeting of the Members in November last, and finally passed without further amendment. Copies of these amended Rules, which are now in force, were issued to Members with the Part of the Proceedings last distributed.

Another important matter which has been settled is the appointment of the first Macleay Bacteriologist. Towards the close of the year the Council again took the matter in hand. Applications for the position were invited by advertisement in Britain and in the Colonies. In response nine candidates offered themselves. The applications were referred to the advisory sub-committee to which matters relating to this appointment have throughout been referred, and a selection of two candidates was made. One of these gentlemen was finally appointed by the Council at a Special Meeting on the 4th inst. The successful candidate is Mr. R. Greig Smith, B.Sc Edin., M.Sc. Durh., F.C.S., who has for some time filled the position of Lecturer in Agricultural Chemistry at the Durham College of Science, Newcastle-upon-Tyne. Mr. Smith comes to us highly recommended from home, and he has had some continental experience in the laboratories of Prof. Stutzer, of Bonn, and of Herr Alfred Jörgensen, of Copenhagen, as well as the opportunity of acquiring some knowledge of the manufacture of tuberculin as carried out on a large scale in the laboratory of Professor Bang, of Copenhagen.

Whether the candidate finally selected should be a Bacteriologist with a pathological bias, or one with a physiologico-chemical, a purely biological, or an industrial bias, were questions which obviously could hardly escape notice and consideration. As matters turned out these questions were settled by circumstances rather than by the direct intervention of the Council. The essential thing is that the Macleay Bacteriologist should be engaged in doing good work. The encyclopædic Presidential Address of Prof. Marshall Ward in the Botanical Section at the recent Meeting of the British Association for the Advancement of Science in Toronto makes it abundantly evident that, over and above purely pathological developments, the operations of bacteria in a thousand ways affect us in matters relating to our daily life, our homes, our food and drink, our domestic animals and our industries. So that here, not less than in the field of infectious diseases, there is ample scope for the investigations of the Bacteriologist who is working only with scientific ends in view.

In this connection, though not a matter directly concerning this Society, it is a matter for congratulation to note the recent appointment of Dr. F. Tidswell, lately Demonstrator in Physiology in the University of Sydney in succession to Dr. Martin, as Bacteriologist to, and the establishment of a Biological and Bacteriological Laboratory in connection with, the Board of Health. This important new departure, taken in conjunction with the appointment of the Macleay Bacteriologist, betokens a noteworthy improvement in the prospects of scientific Bacteriology in this colony.

Passing now from the consideration of the Society's more private concerns, I propose to touch upon one or two outside matters of interest. In October last some of us had the pleasure of boarding the s.s. John Williams to welcome back Professor and Mrs. David and some of the members of the party which visited the island of Funafuti last year for the purpose of putting down a bore in the coral reef. At the time of Professor David's departure from the island, the boring had reached a depth of 557 feet without getting through the reef. Later on Mr. Sweet arrived with the rest of the party, bringing the news that a depth of 698 feet had been finally reached, but without touching bed-rock. We have had the pleasure of hearing from Professor David a general account of his visit; and in a recent number of the Proceedings of the Royal Society of London (Vol. lxii. p. 200, Dec. 1897), will be found his Preliminary Report on the results of the expedition; so that I need not enter into further details. But I cannot allow this occasion to pass without doing what, I feel sure the Society will look to me to do, namely, to tender to Professor David and his coadjutors not only the hearty congratulations of this Society on the success which has attended their enthusiastic labours, but also our earnest wishes for complete success in any further efforts which he may be able to make towards settling this important question.

In the early part of January of this year the Australasian Association for the Advancement of Science held its seventh Meeting in this city. As you already know, the Meeting in the opinion of those best qualified to judge was by no means the least successful and enjoyable of the series. Representative visitors from the other colonies were strongly in evidence, and the opportunities for comparing notes, for exchanging ideas, for talking over questions of correlation and problems of intercolonial and general interest, and for the promotion of good fellowship generally, were freely taken advantage of. The proceedings of Section D., Biology, were somewhat clouded by the lamented death of Professor Jeffery Parker, D.Sc., F.R.S., President elect. Professor C. J. Martin at short notice kindly agreed to fill the breach, and both by his interesting Address, and by his genial and efficient exercise of the functions of Chairman, contributed in a high degree to the success which attended the meetings and deliberations of the Section. On this occasion, for various reasons, the botanists were more strongly and actively represented than the zoologists. They showed their wisdom, too, in having on hand for discussion knotty and perplexing problems of general interest, such as the Classification of the Eucalypts, which can be dealt with to most advantage at the meetings of the Federal Parliament of Science, for then naturalists from widely separated districts in the different Colonies can unburden themselves of their local knowledge, and so contribute to the consideration of difficult questions on broad and comprehensive lines. Zoologists and botanists alike will perhaps be glad to hear that at the next meeting of the Association in Melbourne Professor Spencer hopes in a similar manner to arrange for some special papers leading up to the discussion of biological problems of other than merely local and colonial interest. In no direction perhaps can Section D accomplish better and more useful work.

I trust I may be pardoned for singling out for special remark certain papers which have during the year been published in England, not only since these have been the work of Members of this Society at present absent from Australia but because their subject matter is to a great extent of a very specially Australian interest. I refer to papers by Dr. Robert Broom on the Morphology of Jacobson's Organ in the Mammalia; by Dr. Elliot

Smith upon the fornix cerebri and the margin of the cerebral cortex, on the origin of the corpus callosum and other neurological subjects; and finally by Mr. J. P. Hill, whose luminous paper on the placenta of Perameles will, I am convinced, remain as a classic record of a discovery of the highest biological import.

Not only is the subject matter of these papers largely drawn from Australian sources, but in each case the papers now referred to may be regarded as containing further records of investigations some of whose preliminary results were formerly communicated to this Society and are embodied in its Proceedings.

Lastly, but of the very first importance, there is to be noted the welcome addition to our libraries of a new Text-book of Zoology, in 2 vols., the joint work of two Australasian biologists. We deeply deplore the fact that the production of this great work should have constituted the final episode in the splendid life-work of Professor Jeffery Parker, to whom, together with his distinguished collaborateur, Professor Haswell, F.R.S., a predecessor in this chair, we owe this magnificent compendium of Zoological learning. The book is, I believe, unique in plan and conception. Its unsurpassed wealth of illustration reflects credit alike on authors and publishers, and, along with the pre-eminent excellence of its plan of exposition, must commend it to a place as an educational aid and a general work of reference, hitherto unoccupied, so far as I know, by any other treatise.

I am confident that you all join me in heartily congratulating Professor Haswell on the completion of this great undertaking, as well as upon the recognition by the Royal Society of London of his own acknowledged reputation as a scientific investigator, in his election during last year as a Fellow of that august body.

On glancing around for a subject which I might most suitably take as the leading subject of my address this evening, I early realised that the situation was, for me, by no means an easy one. It so happens that the matters which of late have chiefly occupied my attention are for the most part of such specialised character that the interest they possess for the general biologist is necessarily slight. On the other hand, I have to regret that my own acquaintance with systematic natural history—the aspect of biology which on the whole most directly concerns my fellow-members—is a very narrow one. In view of my positive disqualification from this point of view, I cannot help feeling that my acceptance of the honourable office to which you were good enough last year to call me has placed me in a position which, if not wholly false, is at least somewhat misleading.

I am not in a position to review the recent work in any large division of biological science; nor am I prepared with a contribution towards the advancement of knowledge in any important subdivision of biological inquiry.

How, then, can I best attempt to reveal the intellectual sympathy which yet undoubtedly underlies the relation between us as members of this Society—a sympathy which serves to unite persons of such diverse interests as geologists, physiologists, botanists and entomologists in the common bond of a kindred spirit? Need I do more on an occasion like this than ask you to call to mind the name under which as a Society we are enrolled? For, to the whole civilised world of to-day, the name of the illustrious Swedish Naturalist stands for that of fellowship in that true Nature-worship which consists in lifelong devotion to any one of her manifold aspects, and of which our Society is at once a means and an expression.

The interpretation of the phenomena of life and organisation in some detailed province is what each of us is attempting from day to day, and in his own way, to realise. Yet perhaps it is as well that we should occasionally detach ourselves from the engrossing and fascinating details of our special work, and ask ourselves—not as scientific specialists, but as biologists in a wider sense—what these familiar yet mysterious phenomena of life may imply.

However much the necessities of specialisation may separate us in the everyday aspects of our work, here, at least, we shall be upon common ground. And should such an undertaking require apology, it is that my own qualifications to be the exponent of such topics are so meagre. Yet even this imperfect attempt may be of service in anew directing your own thoughts upon subjects which cannot entirely or for long be kept in the background.

In order to bring under our consideration some of the governing ideas of modern biology, it is well and even necessary to look backwards toward the earlier stages of their growth.

For our present purpose, it is unnecessary to attempt a complete historical retrospect.

From the scientific awakening which characterised the period of the Renaissance up to the early part of the eighteenth century the progress of natural science had been steady and assured.

But when we attempt to realise the state of biological thought in what may be called the Linnean period of the eighteenth century it is necessary to have regard to the conditions imposed upon it by the state of knowledge in other departments, and by the restrictions of a very limited technique of investigation.

It is difficult fully to realise the aspect which the problems of biology presented to men for whom nearly the whole of modern chemistry, and so much of the methods and results of experimental physics, were still non-existent. Microscopy, too, though practised, it is true, as early as the previous century, had made little progress; and though it had been the means of revealing a number of additional structural facts, it cannot be said to have taken rank as a reliable or habitual instrument of research. Of the minute structural characters of living tissue, hardly anything at all was known, whilst the processes and reactions of which these tissues are at once the seat and the essential mechanism were likewise wrapped in the profoundest obscurity. And if these internal relationships of organism were little understood, the interpretation of the external relationship subsisting between organism and organism both in structure and in function was likewise profoundly limited and restricted by the current conceptions of the relations between past and present in the world's history.

The Copernican revolution in astronomy has been rightly regarded as a symbol and an expression of a far more general change, which affected man's entire attitude towards the problems presented by his own being and by the world around him.

The gradual but momentous change in point of view which thus set in revealed itself in many directions, but preeminently in the impetus given to methods of naturalistic interpretation of phenomena.

Closely associated with this tendency was another which made for an "emancipation of our ideas of the past from their bondage to the present" in the interpretation of sequences of events in time. This we may describe as the dawn of the scientific historical method, whose fuller development and wider application to the most varied phenomena has borne such remarkable fruit during the present century.

It was not, indeed, within the domain of the natural sciences, strictly so-called, that the first indications of the development of this method may be clearly perceived. Rather it took form as applied in explanation of the successive aspects of philosophic thought in the eighteenth century. Yet evolutionary science is its flower or fruit; and if this cannot safely be said to have arisen primarily as a biological speculation, it is nevertheless the greatest achievement of modern biology to have provided a detailed demonstration of some of its leading factors and modes of operation in one great sphere of cosmic phenomena.

A brief consideration of the state of biological opinion in the time of Linnaeus may serve to make the subsequent progress clearer. Linnaeus himself was far from being a highly speculative biologist. Preeminently an observer and recorder of facts, his monumental system of classification was admirably adapted to the necessities of his generation. Although substantially a morphological system, based upon facts of structure, the Linnaean classification was artificial as regards its criteria. Still, for Linnaeus himself, those more or less arbitrary structural criteria were only the earmarks, as it were, of a true and actual relationship of the different plants and animals to each other. Such a relationship was conceived by him as indicative of community of origin in the beginning of things in the creative thought of the Maker of the universe. It was not regarded by Linnaeus as an expression of morphological identity of origin by the genuine and natural blood-relationship of descent.

For a considerable period, indeed, Linnaeus maintained the doctrine of the absolute fixity of species. Each species was a final form, a finished product, direct from the hands of the Creator. Yet in later life his views on this question underwent a slight modification. He seems to have held that it was in the genus of to-day that we have to recognise primitive species, and that the differentiation which subsequently ensued was due to hybridisation with other species, thus generating new, but in a sense degenerate, specific forms.

On the whole the views of Linnaeus represent the conservative and non-speculative tendencies of his age. On the other hand, his enormous industry served to accumulate vast stores of those materials which were the essential condition of subsequent progress in scientific hypothesis.

If we wish to gain an insight into the more speculative tendencies of the time of Linnaeus we must turn to his great French contemporary Buffon. The history of the growth and development of the evolution doctrine well illustrates the play of the conflicting tendencies represented by these two distinguished Naturalists. Starting from a similar point of view to that of Linnaeus, Buffon's brilliant imagination enabled him far to transcend the current modes of thought, and in a sense to anticipate several of the future determining ideas of biological science. Not only did he come to doubt the fixity of organic groups, but he anticipated the theory of the action of environment and even dimly the Darwinian doctrine of natural selection itself.

Fertile and suggestive of future advance as his imagination was, Buffon cannot be said to have himself effected any substantial or immediate change in the scientific opinion of his own day. Still the inspiration of his novel and suggestive ideas for some of his successors was a great and lasting one, more particularly and directly upon his younger friend, Lamarck, and also upon Geoffroy St. Hilaire.

When the infallibility of the dogma of fixity of species had been seriously questioned by men like Linnaeus and Buffon, the first and very momentous step had been taken in the direction of the modern standpoint. From this time forth transformist ideas never lacked adherents, though the issue of the conflict with the conservative doctrine of fixity was, owing largely to the later overwhelming influence of Cuvier, for long to remain doubtful. Meanwhile the problem for the transformists became even more complicated. For, supposing it to be granted that structural modification of organic forms has actually occurred, the question then arises: "How, and by what agencies, are we to suppose that this transformation has been effected?" In other words, what are the factors in the hypothetical process of evolution?

In attempting to answer this question the cardinal biological fact of adaptation between organism and its environment stood forth as above demanding recognition and explanation.

How could this harmony or unity be imagined to have been attained and preserved alongside of, and perhaps in spite of, disturbing modifying influences? Two possible answers obviously presented themselves from the naturalistic point of view. Either the direct operation of environment has determined structural change and variation in a passive and plastic organism in the direction of harmony with itself; or, on the other hand, the initiative must in some sense have come from within the organism. The latter must then be conceived as an active agent which, under the pressure of an internal "organic necessity," adapts itself, though in reaction to environment, by actual if slight structural alterations. Further, such acquired changes, the results of constant habitual and useful adaptation to a changed or changing environment, are permanently embodied and handed on to the offspring by inheritance.

The pre-Darwinian evolutionists may be ranked as adherents of the one or other of these explanatory hypotheses. The elder St. Hilaire may represent those who, with Buffon himself, chose the first alternative, whilst the name of Lamarck is now inseparably linked with the second.

It will be observed that in both of these hypotheses thus presented there is presupposed the principle of continuity or uniformity in Nature, which indeed lies at the root of every application of the historical method of interpretation. The negative attitude assumed by Cuvier, the great founder of palæontology, towards the entire theory of mutability is, of course, to be correlated with his advocacy of periodic extinction of types and of catastrophic geological hypotheses generally.

The uniformitarian principle was most strongly upheld by Lamarck, and, though for a time it was relegated to the background by the great authority of Cuvier, it once more, and finally, reasserted itself convincingly in Lyell's Principles of Geology in 1830. From the triumph of uniformitarianism the reassertion of the somewhat discredited evolutionary principle was almost a necessary consequence. Yet Lyell himself was a professed agnostic as to the natural causes determining the successive appearance of new forms; and none of the immediate evolutionist precursors of Darwin were able to add anything new to the discussion of the probable factors and conditions of the process they were disposed to advocate.

The part played by the Darwinian conception of natural selection in gaining for the evolution doctrine a practically universal acceptance in the thought of this century, is too familiar to allow of my pressing it on your attention at any length.

It provided, for would-be evolutionists, that basis of natural causation in organic transformation, the absence of which from the earlier evolutionary theories explains their inability to rise above the almost purely speculative stage. Thus, if we take such speculative evolutionism in perhaps its most striking literary expression, we may recognise in the pregnant thought of Goethe a strong and confident conviction of a unity of type and of a "shaping principle which works underground in Nature." For him these were patent and operative principles, and proofs of actual community of origin amongst organic forms. Yet his suggestive biological ideas were unable to reach the condition of acknowledged scientific certainty in the absence of such a theory of a *modus operandi* in the way of natural causation as is for us supplied by the selection theory.

That theory is indeed the answer to Kant's demand for a "mechanism of Nature" which should "give us an insight into the generation" of organic forms, and should confirm his supposition that these "have an actual blood-relationship, due to derivation from a common parent." That natural selection has justified its claim to be considered as just such a "mechanism of Nature"—as a determining factor in evolution—few if any will now deny. Whether or not, on the other hand, it is an all-sufficient explanation of the appearance of new structural features, and thus of new organic forms, or whether the Lamarckian factor of use-inheritance also plays the part of an integral factor in the process, is even now the subject of most energetic controversy. Into the details of that controversy I do not propose to enter. I would only point out that if the latter factor be admitted to equal rights with the former, the problem of the mode of natural operation, or the mechanism, whereby the effects of use are registered and expressed in definite and transmissible structural alteration, still remains unsolved. But after all this question is not quite a fundamental one. Whether on strictly selectionist principles alone, or with the admission also of use-inheritance, the factor of variation is implied and assumed. Whether, as the selectionist holds, variation is indefinite, and occurs indifferently in all possible directions, or whether, with the Lamarckian, we admit that variation is frequently in a definite and determinate direction, there is yet an element in the chain of natural causation which is fully explained on neither supposition.

It is true that emphasis may be laid, as by Mr. Spencer, and as earlier by St. Hilaire, on the determining effect of environment. But it is next to impossible to prove—and certainly it has not been proved—that simply of itself environment can do anything at all. We can never fully eliminate or distinguish what is due to the reaction of the organism to the environing conditions. Organism is never passive. The distinguishing feature of life consists in activity in the way of adaptation, whether we view it in its internal relations as process in the individual organism, or in the external relations of organism to its outer environment. And if we take refuge, as in the meantime we may still permissibly follow Darwin in doing, in the idea of a "spontaneous variability" of organism, this is of course to confess that we are still unable to penetrate far enough into the ultimate mechanism, if we conceive it as such, which underlies the admitted process of organic modification.

Professor Weismann, it is true, attempts wholly to eliminate the action of environment in the *production* of variations, while assigning to it the exclusive privilege of perpetuating the lucky ones by its selective influence. But it will, I think, be found difficult to do justice to the admitted influence of environment upon the ordinary phenomena of the life of organisms and, as even Weismann admits, upon their somatic structural constituents, and yet jealously and rigidly to exclude these operations from any modifying influence whatsoever upon the germinal constituents. And when even this is actually attempted the resulting effort to account by germ structure for the spontaneous production of the infinite variety necessary for a selection theory, introduces yet another complication into the operations of that tremendous mechanical apparatus of the germplasm, which has been conjured up in explanation of the facts of hereditary transmission. Still the mere fact that complication of this kind is the result constitutes in itself no valid objection to the theory. But, in the last resort, the expedient merely shifts the difficulty of a solution from one sphere to another; and the dexterous compression of the problem so as to enable it to be hidden out of sight in the ultra-microscopical structure of the chromatin of the germ-cell, even if legitimate, can hardly in the meantime be said to make for simplification.

Concerning the details of the argument between Weismann and his critics I shall say nothing. The general verdict amongst biologists in the meantime would appear to be that its results are so far inconclusive. But I may point out that Weismann's contribution to the general theory of evolution may be regarded as a strictly logical continuation of that effort to account for the phenomena of life on the lines of physical causation which the introduction of the conception of natural selection seemed to bring within our reach. In other words, its object is, like that of every purely naturalistic theory, to explain away the teleological phenomenon of adaptation which had appeared to the older biologists to be, *prima facie*, the cardinal characteristic of all organic process. It aims at replacing the idea of purpose or final cause by the purely physical idea of determination by efficient cause as the ordinary and necessary procedure of all scientific interpretation.

This mechanical tendency in the treatment of the relations of the organism to the external world and to other organisms, in space and time, is not its only expression in modern biological thought. On the physiological side also, dealing with life as manifested in the inner relations of the parts and organs of the body to one another, the same spirit has been active.

The vitalistic interpretations and theories which were current earlier in the century have been subjected to a progressive destructive criticism, and it has been claimed that the more insight we get into the true character of living process, the more clearly does it appear that their natural explanation must come to us in terms of physics and chemistry if at all. And there are abundant proofs that the application of physico-chemical ideas and methods to the investigation of vital phenomena is able to carry us further in the direction of an intelligible explanation of living processes than could formerly have been dreamed of.

Whatever may be the final explanation forced upon us of the real nature of the operation of living activity in an animal organism, it is beyond doubt that our acquaintance with the manner of that operation has grown enormously along with the assumption of its essential identity with inorganic process.

Yet in spite of this there have been many indications during the latter part of this century of a reaction away from mechanical and back towards vitalistic interpretation.

It has been contended that, notwithstanding the seeming ease with which many of the phenomena of life can be translated into the language of physics and chemistry, we find whenever we push the analysis of function far enough, that eventually we are simply brought back again to the original problem with which our analysis started, in the ultimate dependence of all bodily process upon the life of the individual component cells of the organism.

Our progress—and after all it is progress—has consisted in pursuing the secret of living activity somewhat deeper into the recesses of organisation. And just when we seem to have eliminated something of the mystery of living process, we find that we have only succeeded in storming the outworks, and that the citadel of the vitalistic position yet lies securely intrenched behind the defences of the living cell.

In other words, the essential problem of physiology has merely been transferred from the cell complex, which forms the body or the bodily organ, to the more remote individual organism or cell, which for us in the meantime forms the unit alike of structure and function.

Even more than this may be claimed by the advocate of vitalism. For the interactions, correlations and co-ordinations subsisting between the component cells and parts of an organism, as in the case of a developing embryo, have not hitherto shown themselves amenable to a mechanical interpretation.

On the other hand, it may be said that recent experimental work on the mechanical conditions of developmental processes is making satisfactory progress in this very direction. And even if we admit that in no case has the progress of physiological investigation enabled us actually to reduce living process to terms of chemistry and physics, this need not blind us to the wonderful and significant advance which the effort to do so has procured. It is not too much to say that every year further facts of organisation and additional events in life-processes are having assigned to them their physical and chemical conditions, and are thus so far being reduced to the position of elements in the cosmos viewed as a mechanically determined material system.

There seems no reasonable ground for believing that the continued application of the same instruments and principles of research, of the same naturalistic conceptions, which has already yielded such magnificent fruit in the proximate interpretation of function and structure, will henceforth become more and more barren. In the struggle after scientific progress what other weapons have we to rely upon? It is significant that, even amongst those who steadfastly deny the sufficiency of chemico-physical interpretations of living process, are to be numbered investigators who have themselves been forward in the application of the most rigidly exact methods of weight, measurement and analysis, in the study of vital phenomena. They have thus done homage to the methods in which the mechanical principle is already in a sense implied, admitting its applicability to certain aspects at least of the phenomena to be investigated.

Is there then any justification for the contention of the "vitalist" of this latest era in physiology? Is there any point at which the principles of physical and experimental inquiry fail in applicability; any aspect of living activity which they are incapable of embodying?

It is claimed, as we have seen, that physiological investigation has not succeeded in eliminating the idea of purpose from the last interpretation of any biological fact of structure and function which has been offered for analysis.

That science will ever enable us to say that at last we have a perfect, self-consistent and complete mechanical explanation of even the simplest fact of living process or tissue seems to me to be in the highest degree improbable.

The brilliant physiological analysis of the mode of working of the bodily organs which is one of the characteristic products of the biological activity of the century has indeed by no means ceased. But though still proceeding in manifold and specialised directions, it is hampered at almost every turn by the difficulties attaching to an explanation of the living activity of the cell unit.

And if, to-morrow, this obstacle be partly surmounted—as is likely enough—by further discoveries in the way of intracellular mechanisms than the important ones already made, yet we may be very sure that in every forthcoming interpretation the notion of adaptation or purpose will again re-assert itself, though for a time it may be concealed under the disguise of a mere unexplained residuum which refuses to be read into the next current mechanical hypothesis.

Does it not appear to be the doom of Biology to be for ever endeavouring to reduce such an unexplained residuum? It must never despair of its ability to translate the facts into the language of physical causation. Thus only does it fulfil its mission as a branch of Natural Science which is "to distinguish the threads of necessity that bind together the most disparate phenomena" even though in so doing it may seem to be "explaining away all life and unity in the world and putting everywhere mechanism for organism even in the organic itself."

But we are by no means compelled to assume that the method of explanation thus pursued represents the only mode of apprehension of the facts, the only possible interpretation of their meaning. It is indeed vain to look to Science for the recognition of an aspect of living phenomena which it must of its own inner necessity ignore. On the other hand, "there is little ground," said Prof. Burdon Sanderson in 1889, "for the apprehension that exists in the minds of some that the habit of scrutinising the mechanism of life tends to make men regard what can be so learned as the only kind of knowledge. The tendency is now certainly in the other direction. What we have to guard against is the mixing of two methods, and, so far as we are concerned, the intrusion into our subject of philosophical speculation. Let us willingly and with our hearts do homage to 'divine philosophy,' but let that homage be rendered outside the limits of our science."

It is just such an intrusion of the fruits of a distinctively philosophical interpretation of organisms into the domain of strictly scientific speculation that tends to vitiate the modern "vitalistic" views. I am willing to admit that the vitalistic recognition of purpose does, in a sense, more justice to the facts of organism than a method which ignores purpose. But I do not think the idea of purpose helps us at all in strictly scientific and experimental procedure, and its attempted scientific application is simply an attempt to "find a gap in that circle of mechanical motions" which alone constitutes the cosmos for experimental science.

In science properly so called, "the phantoms of life, the final causes" which (as Mr. Caird says in this exact connection) "distort the prose of science" must be resolutely put from us, even though, with them, all hope of finality and unity in the ultimate explanation of the world, from the point of view of physical science, completely disappears.

In a genuinely scientific explanation there is never reached a stage at which we can forsake the mechanical method simply because we can no longer recognise, nor easily imagine, the nature of the unknown antecedents of a phenomenon. Vitalistic or teleological interpretation is not a method which comes to our rescue when a physical interpretation fails us. In so far as it is valid at all, it is one which is present with us and which urges itself upon us *at every stage*, forbidding us ever to mistake a possible mechanical interconnection of the phenomena of life for the real ground in thought of purposive adaptation. This idea indeed intrudes itself upon our apprehension as the special characteristic of the organic world at any and every stage of scientific development, but it is not a product of the *scientific* imagination. Any apparent force which latter-day vitalistic objections to the mechanistic procedure of science may possess would seem to depend upon the mixing up of two possible modes of explanation. The endeavour is made, by pointing to the incompetence of the mechanical method to explain certain aspects of living process, to make room within the circle of scientific experience itself for a mode of explanation which has neither relevance nor validity in the sphere of experimental science.

It is doubtless true enough, as the vitalist maintains, that it is insufficient as an explanation of living process merely "to trace energy from the surroundings through the organism and out to the surroundings again. If," he continues, "this is to be taken to be a full account of the process it is inadequate, for it ignores the fact, characteristic of life, that the energy spent by the organism on its surroundings is not dissipated at random on those surroundings, but is so directed as to cause them to give back again to the organism, sooner or later, just as much energy as the organism has previously expended. In other words, the distinguishing feature of vital activity is self-preservation or the conservation of the organism in a state of functional activity."

The criticism is just, and appropriate enough. But from the strictly scientific point of view the fact that there is not an indefinite, but a definite distribution of energy simply suggests a further search for a mechanism to account for this additional fact of distribution along lines which, as a matter of fact, make for self-preservation. It will not do to say that such a mechanism is inconceivable. It was just such a problem with which Science was confronted during the growth of the theory of Evolution. How was the obvious adaptation of evolving organism to environment to be accounted for on the lines of Natural Causation? The answer to this was the theory of Natural Selection.

And just in so far as the Natural Selection theory eliminates the idea of purpose (contained in adaptation between organism and environment) from the notion of Evolution, so far also—and no further—might a possible extension of mechanical hypothesis enable us to dispense with the idea of final cause suggested by the purposive distribution of bodily energy above referred to.

It will not do to harbour the notion that the current of energy, of which the organism is conceived as the physiological channel, can be either interfered with, or even determined in its direction by, purposive conditions. So to represent it is found to involve the vain attempt "to get at an end or final cause without leaving the point of view of efficient causality." And, just as determinism is within its rights in abolishing the abstract self "which claims an empirical freedom of will amid the strife of motives," and as it is impossible "to save for this self even the power of directing attention on one motive rather than another," so, whenever the organism is regarded as a vehicle of energy, it is vain to aim at vindicating the idea of final cause by claiming for it any empirical power of determining the distribution of that energy for ends or purposes.

In the same able essay from which I have quoted the reference to purposive distribution of energy, allusion is made to the well-known phenomena manifested in the regeneration of the amputated limb of a newt. After summarising the process by which the bud of embryonic tissue goes on to re-form all the tissues of the lost limb, bone, muscle, nerve, &c., the writer proceeds:— "Every cell performs its appropriate duty until the whole business is accurately finished without fail. Is it conceivable that each of the thousands of separately existing cells concerned in the process should have a mechanism within it which would cause it in spite of all obstacles to take up the position and undergo the modification requisite for the proper performance of its work in the newly developed hand? Or is it conceivable that mechanical pressure of any kind should cause the bud to grow into a perfect hand? The alternative hypothesis is that each cell is determined directly in its action simply by what it has to do in order that the vital activity of the newt may be restored to its normal condition."

Now to my mind it is not only not impossible but it is almost imperative that we should conceive just such mechanical arrangements as are here assumed to be out of the question. Without such presupposed mechanism no conception of detailed sequences of events could be formed and the entire natural process would have to be regarded as physically unintelligible. But some definite chain of physical events in such a case there *must* be; and each event must have its physical antecedents and conditions which must almost necessarily be embodied in some sort of structural mechanism. What that mechanism is is of course precisely the kind of question which it is the function of Natural

Science to ask and her chief business to answer. To the question whether in such a case an answer can even be conceived it ought to be sufficient to reply that of recent years it has been the object of Weismann's elaborate theory of the architecture of the germ plasma to furnish just such an answer. Whether the effort is well or ill-directed to that object it is beside the question to enquire. If not that solution then another, not less mechanical, may be forthcoming.

We may therefore pay little heed to those who would bid us cast away the hope that the closer investigation of cell structure and function may enable us to read even these into the convenient if more abstract terms of mechanism. It does not follow that the mechanism itself will be found to be simple. The nucleus of an ovum, so long as we can say little or nothing of its structure, seems an object of no great complexity. But if we are to make any progress at all on naturalistic lines, the future advance of biological investigation must consist in unravelling the enormous structural complexity with which we are bound to credit it. And as an attempt in this direction even the demand made on the mechanical imagination by Weismann's stupendous germ plasma may be regarded as not greatly excessive. Such an hypothesis as Nägeli's micellar theory too might likewise open up a most fruitful field of discovery.

It appears to me most probable that ere long we shall arrive at ideas with regard to the architecture, not only of the germplasma, but of the cell as a whole on the lines of some such conceptions as are involved in theories like those of Weismann and Nägeli.

Nor need we pay great heed to the warnings we sometimes hear respecting the bounds to further structural investigation imposed by unavoidable optical limitations, as in the construction of lenses.

It may be true that by-and-bye we shall reach such optical limits. But the implied assumption is hardly warranted, that only by optical means and methods can we possibly in future gain an insight into what we now term the ultra-microscopical structure of living tissue. It is surely quite amongst the practical possibilities of future science that the arrangement in space of the material particles of protoplasm supposed, for example, by such an hypothesis as Nägeli's, may be sufficiently attested and verified by other than optical means; it might even be by the incidence on appropriate instruments of other than optical radiations. Who can tell what structural facts may not be borne witness to by future instruments of research?

It seems reasonable to believe that no limit can be assigned to the efforts of science to supply an answer to all questions relating to the "how" of phenomena—to the manner of their being and becoming, past and present.

As to their "why,"—their object, purpose or final cause,—that is sometimes declared to be a matter of which we are not only ignorant, but of which we cannot even hope ever to know anything at all. And if what I have already said be true, then it follows that upon such questions Science in the narrower sense must be for ever dumb. We must be content to recognise that its operations are conducted entirely on the plane of a mechanical interpretation of phenomena even when its subject matter consists of organised material and living process.

What place, then, can be assigned to the notion of purpose or final cause in a scheme of human knowledge? Is there any sense in which its validity in the interpretation of the world must be acknowledged? Thus stated, the question need no longer excite the suspicion with which any claim on the part of teleology to strictly scientific validity must be viewed.

It cannot be denied that the adaptation of objects and processes to ends or purposes is plainly and unmistakably suggested to the ordinary human intelligence. It is true that this suggestion is not obviously pressed upon us by a consideration of the facts of the inorganic world. But whenever we enter the domain of organism we find, even in the lowliest expression of living activity, that we can no longer ignore the purposive character of that activity. We seem to have entered upon the exploration of a kingdom of ends, wherein all events that occur suggest not merely, or even chiefly, a dependence upon preceding events, but a dependence upon events which have not yet occurred. In other words, organisms appear to perform acts in order that more or less definite results may be brought about; and the nature of the living acts is therefore determined not merely by what has gone before, but by what is yet to follow. "It is that which is about to be that guides the growing thing and gives it unity."

It is this adaptation of means to ends which is put in the forefront in all teleological interpretation. And a very little consideration is sufficient to convince one that this notion of the determination of means by ends not merely differs, but is radically distinct from, that of physical determination by antecedent phenomena. It amounts to a complete inversion of the order of physical causation.*

To assume that since the idea of determination by ends involves a point of view essentially distinct from that of efficient causation the notion of end or purpose must therefore be put aside as a mere preliminary illusion of the intelligence—as a fiction which we accustom ourselves to suppose—is simply to beg the question.

The validity of this or that principle of explanation cannot be decided in a rough and ready fashion. It is not a question simply of the relative success of either principle in enabling us to string facts together in a more or less intelligible order. Both principles may assist us in doing so, and may thus claim to be so far regulative of experience.

To decide upon the limits of the validity of each and all of such principles or categories of explanation is the paramount function of a genuine philosophical criticism. It is to this that

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* It is idle to fall back upon Hume's supposed metaphysical elimination of the idea of necessary connection, causal or other, in order to get rid of the difficulty raised by this distinction between efficient and final cause. This destructive criticism is quite as effective in destroying the foundations of ordinary scientific reasoning as in getting rid of the teleological conception. And it has been abundantly shown that on such a basis of philosophical scepticism as to the fundamental conceptions, *e.g.*, of cause and of substance, no system of human knowledge can possibly rest. —*Cf.* Green's Introd. to Hume's Treatise on Human Nature.

we must look to enable us to determine the relation to the whole of human experience of any one of the principles which appear to be implied in that experience. Before this tribunal the competing claims of teleology and purely physical determinism, as principles explanatory of Nature, must ultimately be brought. And when this is done it will invariably be found that it is impossible to allow the discussion of the fundamental conceptions of knowledge, like those of substance and cause, to proceed merely with reference to the phenomena of Nature conceived objectively.

In every criticism of the nature of knowledge which is not wholly superficial it will be found that there is involved a reference and a relation to the self-conscious subject of knowledge as the indispensable condition of all experience whatsoever.

This is neither the time nor the place to attempt to set forth what I take to be the results of such a criticism of the conditions of knowledge. I can only permit myself to affirm my own conviction that an impartial study of the problem thus suggested will result in a recognition that the conception of the cosmos—the object of human experience—as a mere system of material and mechanical relations in space and time is after all highly abstract and unreal. For certain purposes such a conception may be not only useful but indispensable, just as are the professed abstractions of mathematical science. But the hypothesis which regards the cosmos of experience as reducible to an endless series of phenomena in time and space, connected by a common bond of external necessity, entirely ignores the fundamental relation of all fact whatever to a knowing subject as the essential condition of all experience. No hypothesis which abstracts from this reality can possibly claim to offer a satisfactory interpretation of things. And it will be found whenever full recognition is afforded to the one inalienable condition of experience, that, amongst other ideas, that of final cause or purpose must be reinstated as a valid and necessary principle of explanation in any philosophical interpretation of the world.

It is a consequence of the acceptance of such a philosophical doctrine that although, even in biology, we must, if we wish to make progress on truly scientific lines, continue to bring all the facts of observation and experiment under the dominant idea of mechanism or physical causation, yet we are continually forced to recognise the incompetence of the mechanical principle to satisfy the intellectual demand for a full comprehension of the significance of living process. And this inadequacy becomes the more glaring as the phenomena to be investigated approximate more and more to the character of manifestations of conscious intellectual activity.

The difficulty arising out of the confusion of two points of view, emerges in one of its most impressive and characteristic forms in the efforts to apply the principle of evolution, in its guise as a principle of natural history, to the manifestations of human activity in social institutions and laws of conduct.

It has indeed been one of the triumphs of the historical method to have largely "emancipated our views of the past from their bondage to the ideas of the present" by means of "the conception of the evolution of man by interaction with his environment."

In its more extreme form, however, this idea of human evolution has been interpreted on the lines of organic evolution generally, as a sequence of natural phenomena causally connected by the aid of the principles of variation, heredity and natural selection.

Earlier in this address I have referred to the representation of natural selection operating upon indefinite variation, as a means of explaining organic adaptation as a purely naturalistic process.

Even as applied to the lower stages of organisation, we saw that this reduction could not be regarded as having been actually effected, so long as the residual phenomenon of variation remained unexplained. Evolutional adaptation still remains dependent upon an inherent "spontaneous," or at least an unexplained variability.

And when we come to apply the conception of evolution to the products of conscious human activity, we find ourselves upon still more uncertain ground.

The late Prof. Huxley, in the last of his memorable and striking utterances, once again proclaims his deeply-rooted faith in the ultimate unity of all "cosmic process," expressing itself in secular evolution But he found himself, nevertheless, compelled to postulate within this process a kind of countermovement as regards natural selection, when he is considering certain aspects of human evolution.

"The faith that is born of knowledge," Prof. Huxley says, "finds its object in an eternal order, bringing forth ceaseless change through endless time, in endless space; the manifestations of the cosmic energy alternating between phases of potentiality and phases of explication."

The aspect of cosmic activity which the great apostle of evolution singles out for special treatment in the essay to which I allude, is what we may call the human episode in the cosmic process. This episode, you may remember, he sets forth under the metaphor of a garden, cut off from the unreclaimed bush of general cosmic activity, and tended, watered, and otherwise protected from the incursions of wild animals and the hurtful competition of noxious and undesirable plants. He is not concerned with the origination of the garden, for obviously this must be regarded as in some sense due to the operation of the ordinary laws which govern the entire region. The domesticated area must in some natural way have become shut off from the wildwood. But he is specially concerned with the fact that, given such a garden, the denizens of it are now largely protected from the operation of the ordinary natural and competitive conditions prevailing outside its limits. By this he attempts to convey the notion that one aspect of the result of human evolution by natural selection has been the limitation, within the garden of human society, of the operation of those very conditions of struggle and survival to which its genesis is owing. And he accordingly proclaims the "apparent paradox" that "ethical nature while born of cosmic nature is necessarily at war with its parent."

No scientific writer of modern times has exhibited a greater mastery of apposite and forcible metaphor than Mr. Huxley. But there have been occasions like that I now refer to when the metaphor is so forcible that it appears to carry off its author bodily.

If ethical process is really the legitimate offspring of the cosmic process, then all the features subsequently revealed in the former have surely a full hereditary title to the name and privileges of the parent. And indeed Mr. Huxley was forward to remark that none was more willing than he to admit the ultimate identity of the two kinds of process. And yet he immediately pushes the idea of the war between offspring and parent so as to warrant the conclusion that the processes somehow become essentially distinct.

It is by no means hard to perceive that the source of the so-called paradox is to be found in Mr. Huxley's identification of "cosmic process," in its evolutionary aspect, chiefly if not entirely with the principle of natural selection. And of course when he goes on to recognise that a condition of human progress on the ethical side has been a restriction and limitation of the struggle for existence amounting almost to the suppression of its internecine features, he is constrained to express the difference as a war between parent and child, between the cosmic and the ethical processes. But surely, and I say it with all respect, this is the most utterly obvious fallacy. *Either* the forms and institutions of ethical activity are a non-natural product, and no genuine daughters of the cosmic process, or else they are as much *cosmic* in their origin and essential nature as are the satellites of Jupiter. How can they, from Prof. Huxley's point of view, ever cease to be cosmic or even begin to become anything else?

Such is the preliminary difficulty or confusion. Let us see how it works out in other directions. Allusion is made to "bee society" as a somewhat analogous phenomenon to that of human society. "Bee society," we are informed, "is the direct product of an organic necessity impelling every member of it to a course of action which tends to the good of the whole. Each bee has its duty and none has any rights." [Has not the queen as much right to her special appointments as any human monarch to the regal accompaniments of his function? At any rate, if we cannot here speak of a "right," as little can we talk of a "duty."] "In the same sense as the garden or the colony is a work of human art, the bee polity is a work of apiarian art brought about by the cosmic process working through the organisation of the hymenopterous type." Again, he says, "I see no reason for doubt that at its origin human society was as much a product of organic necessity as that of the bees." Then he points out that self-assertion in man is a survival of the original "organic necessity" out of which human polity arose, and that certain "organic necessities" operate as checks upon this "self-assertion," as, for example, family affection, sympathy, &c. "We come to think," he continues, "in the acquired dialect of morals." "An artificial personality, 'the man within,' as Adam Smith calls conscience, is built up beside the natural personality. He is the watchman of society, &c., &c." He then goes on:—"I have termed this evolution of the feelings out of which the primitive bonds of human society are so largely forged, into the organised and personified sympathy we call conscience, the 'ethical process.'" But since Prof. Huxley has already taught us to regard this as the natural offspring of the cosmic process arising at the stage of organic necessity, whence comes the arbitrary distinction between the one as "natural" and the other as "artificial?" Surely, the identity of origin forbids us to pit the one against the other as of alien growth! The ethical, if recognisable at all, is "cosmic" through and through, and it is vain to talk as if they were each manifestations of distinct principles.

In the treatment of bee polity, the explanation offered is that it is "a product of an organic necessity impelling every member of it to a course of action which tends to the good of the whole."* Yet when a not dissimilar limitation of the struggle for existence amongst the individuals comprising human society has to be

* This is of course pure "cosmic" activity.

characterised, it is no longer "cosmic," but "ethical," conceived as directly antagonistic to the former.

I have criticised these views at length because I think it is plain that the source of the confusion is that arbitrary identification of organic "cosmic" process with the process of Natural Selection on a basis of struggle with elimination of the unfit. Now, Mr. Huxley's proclamation that this principle does not prevail in an unmodified form in human society; and even that, to a large extent, the progress of human society does not depend upon the struggle for existence, is tantamount to a declaration that Natural Selection is *not* the sole and only factor in the movement of the cosmic process. For it is strictly inevitable that we should take the latter in the full and only legitimate sense as embracing the entire conditions of the ethical process as fully as it does the necessities, organic or other, which direct and control either bee society or planetary movements.

I am not here concerned to inquire whether or how far human progress as a manifestation of "ethical process" is characterised by such a suppression of the struggle for existence as has been insisted on. Whether there is substituted for it, in the later phases of human evolution, a struggle for the means of enjoyment, as Mr. Huxley held, or a struggle for existence, with survival of the fittest, not of individuals, but of ideals of action, as Mr. Ritchie believes, is also a matter which may be left undiscussed.

But the admission that the mere extension of the Darwinian theory of natural selection is not fitted to account for the evolution of human society and institutions, at least in the later phases of that process, is one which, as coming from Mr. Huxley's maturer thought, cannot be lightly passed over.

The fact is that when we reach the higher planes of "cosmic process," including in this term the "ethical" element with which Mr. Huxley can only be said to juggle, we find,—not indeed a reason to deny the applicability of the methods of explanation which have proved useful in dealing with simpler phenomena—but that these are no longer to be recognised as capable of satisfying the intellectual demand which the situation makes upon our thought. Such satisfaction as they convey is but formal. The thirst for explanation of the really significant aspect of the complex phenomena of human activity remains practically unquenched.

There may be a relative truth in such a statement as that the phenomena of human history and conduct, the manifestations of the human spirit in art and literature, and of such thought-products as pure mathematics or the more concrete sciences, may be viewed as products of physical sequences in the way of redistributions of matter and energy. In a sense, again, we may be entitled to say that the human events thus conceived have been manifested and epitomised in a structurally variable germplasm, perpetuated by natural selection, and unfolded and brought to fuller fruition as episodes in the functional activity of the modified protoplasm of nerve tissue.

I neither doubt the possibility nor deny the desirability for certain purposes of naturalising in this way the facts and processes of conscious human activity.

Every mode of explanation is relative to a certain point of view. Thus, it will be generally admitted that the hypothesis of human society as constructed solely on the basis of the idea of wealth is incompetent fully to explain the concrete phenomena either of individual or of a corporate social and national life. Yet the science of political economy which to a large extent depends on such an hypothesis has nevertheless its own value and function. Or again, I may borrow an illustration from an essay from which I have already quoted, and point out that "no physicist really supposes that he is dealing with anything else than a metaphysical abstraction as distinguished from a real object, in a purely kinematical investigation."

But the utility of such admittedly provisional hypotheses becomes ever the less the more the obvious complexity of the actual fact obtrudes itself upon our mental horizon. In spite of ourselves our point of view becomes altered; and it is no small part of the discipline of the scientific intelligence to avoid the confusion of different categories of explanation; to "put himself aside and let Nature speak," Nature, that is to say, which is for him a purely mechanical system.

And it is just such a confusion of thought which on the other hand permits the presentation of scientific and physical formulas as if these exhausted the reality of living or conscious activity or were other than lame and often grotesque travesties of the actual content of the phenomena in question.

I have already tried to show that at the root of the modern doctrine of natural selection (survival of the luckily endowed) there lies the mechanical principle of external necessity in a determining environment. I have indicated my conviction that it is this aspect of it which vitiates its attempt to explain by itself the ethical aspect of human evolution, and which seemed to give point to the self-contradictory notion of a conflict between the cosmic and the ethical principle.

The *fact* of a continued process of human evolution cannot be withstood. But we may readily follow Mr. Huxley in his assertion that natural selection does not satisfactorily account for the later phases and stages of it. If, then, we are to retain our grasp of the essential identity of all cosmic process, we must be prepared to recognise that if the end is not intelligibly to be conceived as mere mechanism neither can the beginning be so explained.

And what is true in relation to the ethical aspect of cosmic process as revealed in human society, is true also of the organic aspect of that process as revealed in plant and animal life. The mechanical interpretation is only a convenient, a provisional, above all a working, hypothesis. As a final or philosophical interpretation it is false, because it ignores one, and that the really significant aspect of the facts viewed from the general philosophical point of view.

And, exactly as in the case of the ethical process, it does not help us much that we are able, by the aid of the doctrine of evolution, to trace back the series of living forms to their simplest, most formless, and structureless beginnings. "The continuity of all existence," which is the essence of the evolution idea, "may be interpreted," says one writer, "in two very different ways. It may lead us either to radically change our notions of mind and its activities, or to 'radically change our notions of matter.' We may take as the principle of explanation either the beginning or the end of the process of development. We may say of the simple and crass, 'There is all that your rich universe really means'; or we may say of the spiritual activities of man, 'This is what your crude beginning really was.' We may explain the complex by the simple or the simple by the complex."

"And one of the most important questions for morality and religion is the question, which of these two methods is valid. If out of crass matter is evolved all animal and spiritual life, does that prove life to be nothing but matter; or does it not rather show that what we, in our ignorance, took to be mere matter was really something much greater? If 'crass matter' contains all this promise and potency, by what right do we still call it 'crass'"?

"It is manifestly impossible to treat the potencies, assumed to lie in a thing that grows, as if they were of no significance; first to assert that such potencies exist, in saying that the object develops; and then, to neglect them, and to regard the effect as constituted only of its simplest elements. Either these potencies are not in the object, or else the object has in it, and is, at the first, more than it appears to be. Either the object does not grow, or the lowest stage of its being is no explanation of its true nature."

In this way may a perfect loyalty to the evolution doctrine throughout the entire domain of cosmic process, from its lowest to its highest manifestations, bring with it an emancipation from bondage to those mechanical principles which seem alone suggested on the lower plane of the inorganic and which may, for certain purposes, though with more conscious effort, be applied throughout the whole sphere of objective science.

On the motion of Mr. Henry Deane, M.A., a most cordial vote of thanks was accorded to the President for his interesting Address.

The Hon. Treasurer read his final report on the Society's financial condition and outlook, and presented his accounts and balance sheet, duly signed by the Auditors as correct. From these it appeared that the balance standing at the credit of the Society on both Income and Bacteriology accounts was £607 14s. 2d., but that when the Society's income account only was considered, there was a small excess of expenditure over income for the year amounting to £6 3s. 11d.

On the motion of Rev. J. M. Curran, seconded by Mr. W. W. Froggatt, the Hon. Treasurer's report was adopted.

On the motion of Mr. J. R. Garland, seconded by Mr. W. S. Dun, a resolution expressive of the Society's regret at Dr. Norton's retirement from the office of Hon. Treasurer, and of its weighty obligations to him for his valuable services during a period of sixteen years, was carried with acclamation.

The following gentlemen were elected to fill eight vacancies in the Council:—Professor J. T. Wilson, M.B., Ch.M. (President), J. C. Cox, M.D., F.L.S., Thomas Dixson, M.B., Ch.M., Prof. W. A. Haswell, M.A., D.Sc., F.R.S., Hon. James Norton, LL.D., M.L.C., Perceval R. Pedley, Prosper N. Trebeck, J.P., Walter W. Froggatt, F.L.S.

And as Auditors: Hugh Dixson, J.P., Edward G. W. Palmer, J.P.

ENDOWMENT (CAPITAL).

	£	s.	d.		£	s.	d.
Amount received from Sir William Macleay in his life-time	14,000	0	0	Loan A	3,000	0	0
Further Sum bequeathed by Will, £6,000, less Probate Duty	5,700	0	0	Loan B	5,000	0	0
				Loan (portion of Loan C, secured with other money by mortgage for £24,000)	11,700	0	0
	£19,700	0	0		£19,700	0	0

BACTERIOLOGY (CAPITAL.)

	£	s.	d.		£	s.	d.
Legacy of £12,000, bequeathed by Sir William Macleay to the University of Sydney (less £600 probate duty), paid by the University into Court and ordered to be paid out to the Society	11,400	0	0	Loan (portion of Loan C)	11,400	0	0
Amount (out of interest received) ordered by the Council to be added to Principal	700	0	0	Fixed Deposit in Commercial Bank for 12 months, from 2nd Dec., 1897, at 3 %	700	0	0
	£12,100	0	0		£12,100	0	0

March 8th, 1898.

Audited and found correct.

E. G. W. Palmer, Auditor.

James Norton, Hon. Treasurer.

ENDOWMENT (INCOME).

	£	s.	d.		£	s.	d.
Balance from 1896	157	11	11	Error in calculation of Interest received for Bacteriology Income account for 1896	5	17	2
Entrance from five Ordinary Members ...	10	10	0	Plates and Illustrations	189	0	0
Subscriptions from sixty-four Members ...	82	2	0	Printing Proceedings and Sundries ...	294	0	10
Interest received	861	15	0	Postage (Hon. Treasurer's)	1	0	0
Sales of Proceedings	12	13	5	Ground Rent, Rates and Insurance ...	84	4	7
Balance due to Bacteriology Income Account	6	3	11	Bank Charges on Country Cheques ...	0	12	10
				Petty Cash voted by Council (Advertisements, Postage, Gas, Distribution of Publications and Sundries)	50	0	0
				Salaries and Wages	476	0	0
				Books and Binding	15	7	6
				Telephone	10	0	0
				Shipping Charges, Insurance, &c. ...	4	13	4
	£1,130	16	3		£1,130	16	3

March 8th, 1898.

Audited and found correct.

E. G. W. Palmer, Auditor.

James Norton, Hon. Treasurer.

BACTERIOLOGY (INCOME.)

	£	s.	d.
Balance as per Statement of 1896	1,717	2	11
Error in Statement of 1895	5	17	2
Twelve months' interest at 4 % on £11,400, part of £24,000 (Loan C) to October 30th, 1897	456	0	0
Twelve months' interest on £900 lent out of income, also forming part of Loan C	36	0	0
	£2,215	0	1

	£	s.	d.
Advertisements	1	2	0
Interest added to Capital, on deposit in Commercial Bank	700	0	0
Balance covered by £900, lent on mortgage (part of Loan C); by £607 14s. 2d. standing at the Society's credit in Commercial Bank; and £6 3s. 11d. due by Endowment Account (Income) ...	1,513	18	1
	£2,215	0	1

March 8th, 1898.
Audited and found correct.
E. G. W. Palmer, Auditor.

James Norton, Hon. Treasurer.

F. Cunninghame & Co., Printers, 146 Pitt Street, Sydney.

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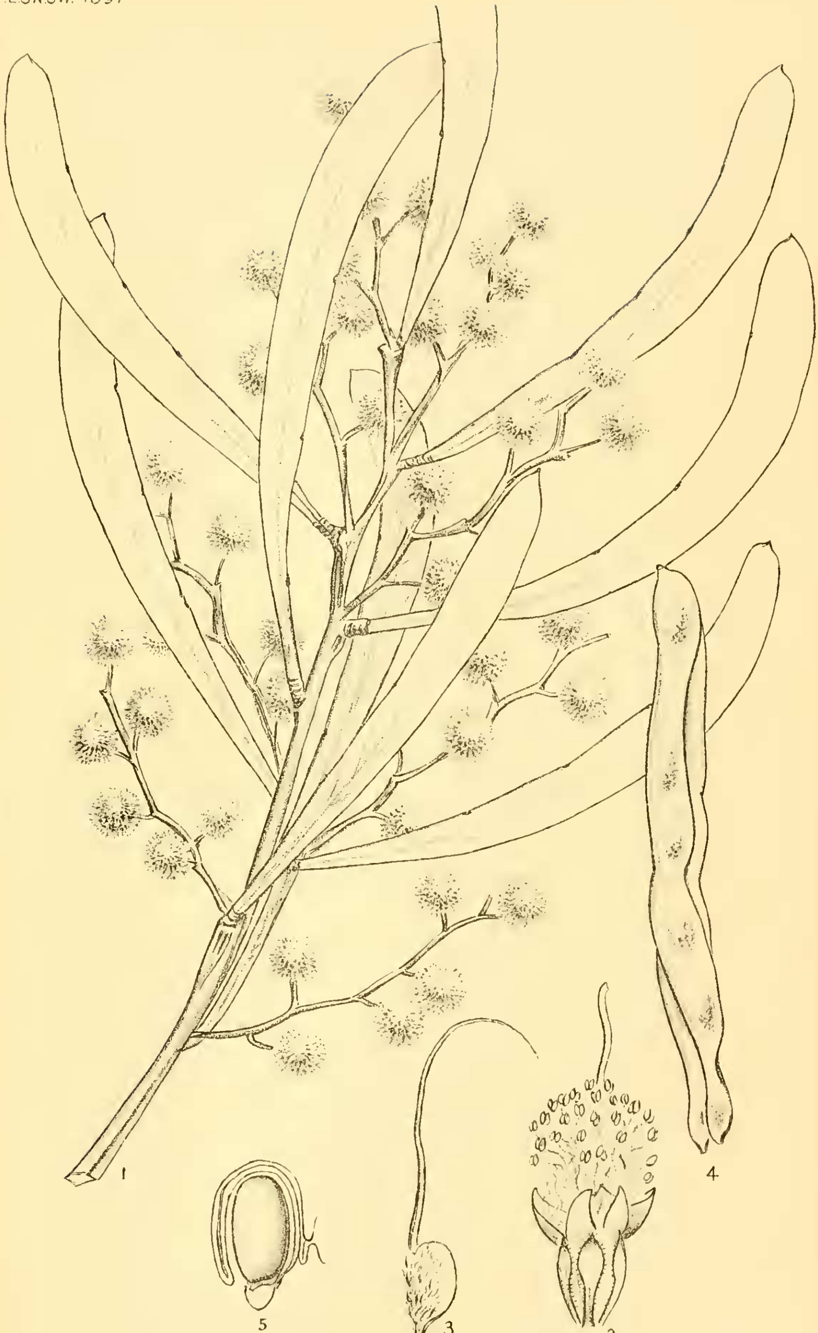
(1897.)

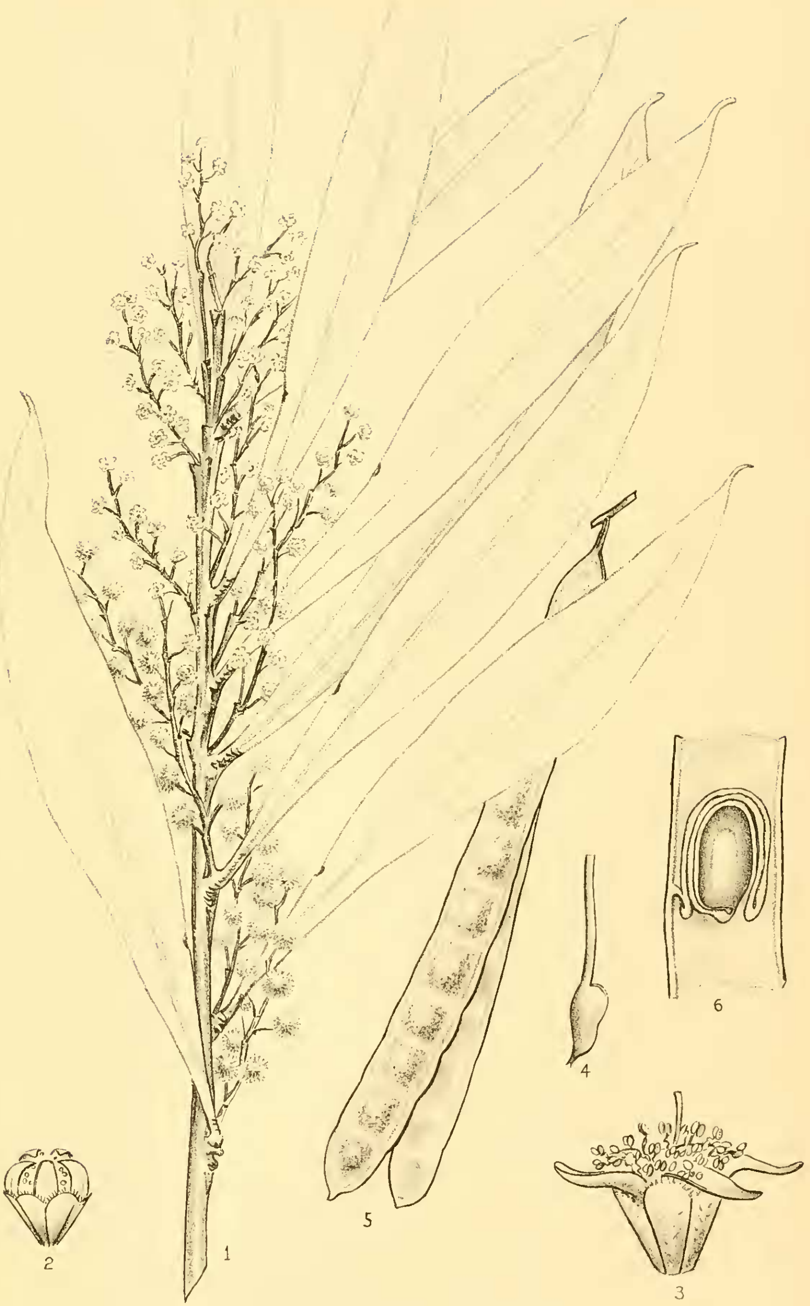
Names in Italics are Synonyms.



ACACIA GLADIIFORMIS, A. CUNN.

R.T.B. del.

ACACIA OBTUSATA, Sieb.



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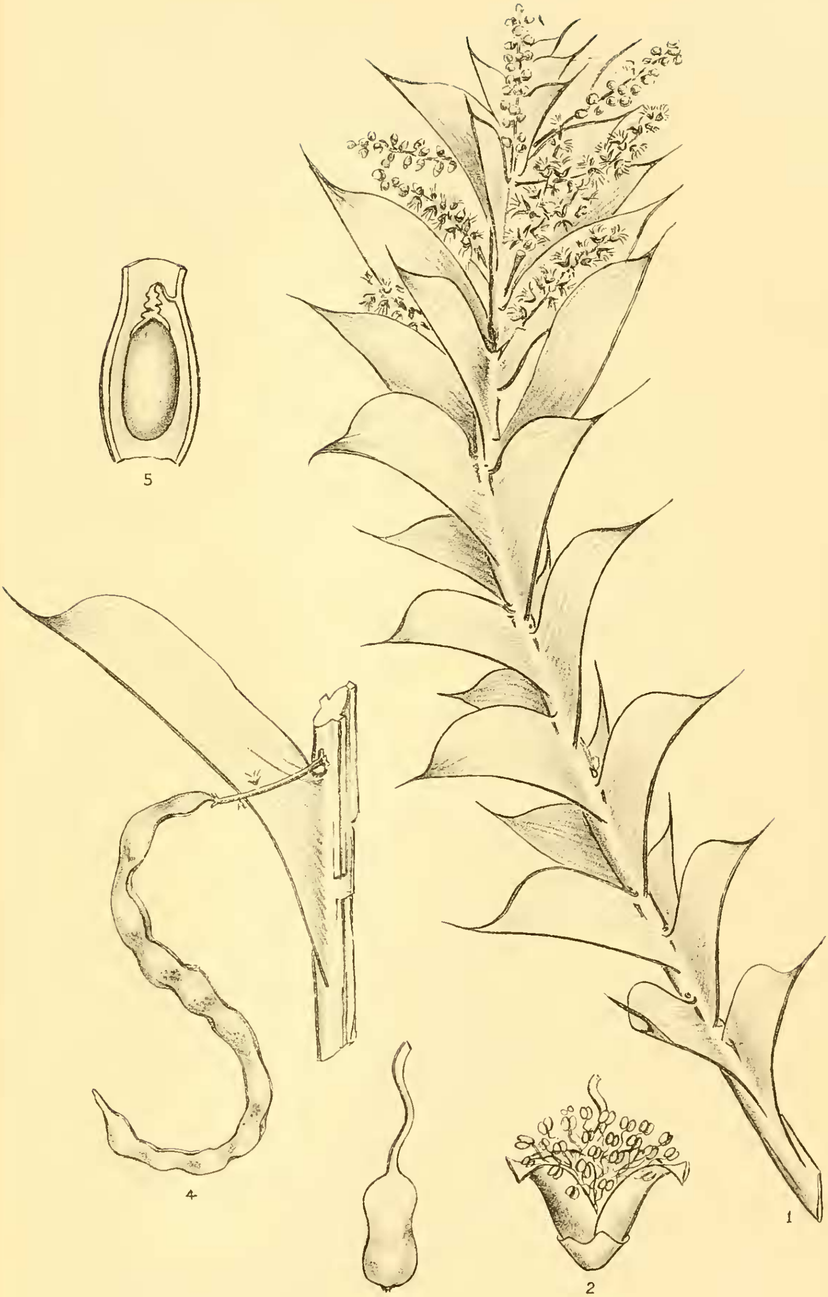
ACACIA RUBIDA, *A. Cunn.*



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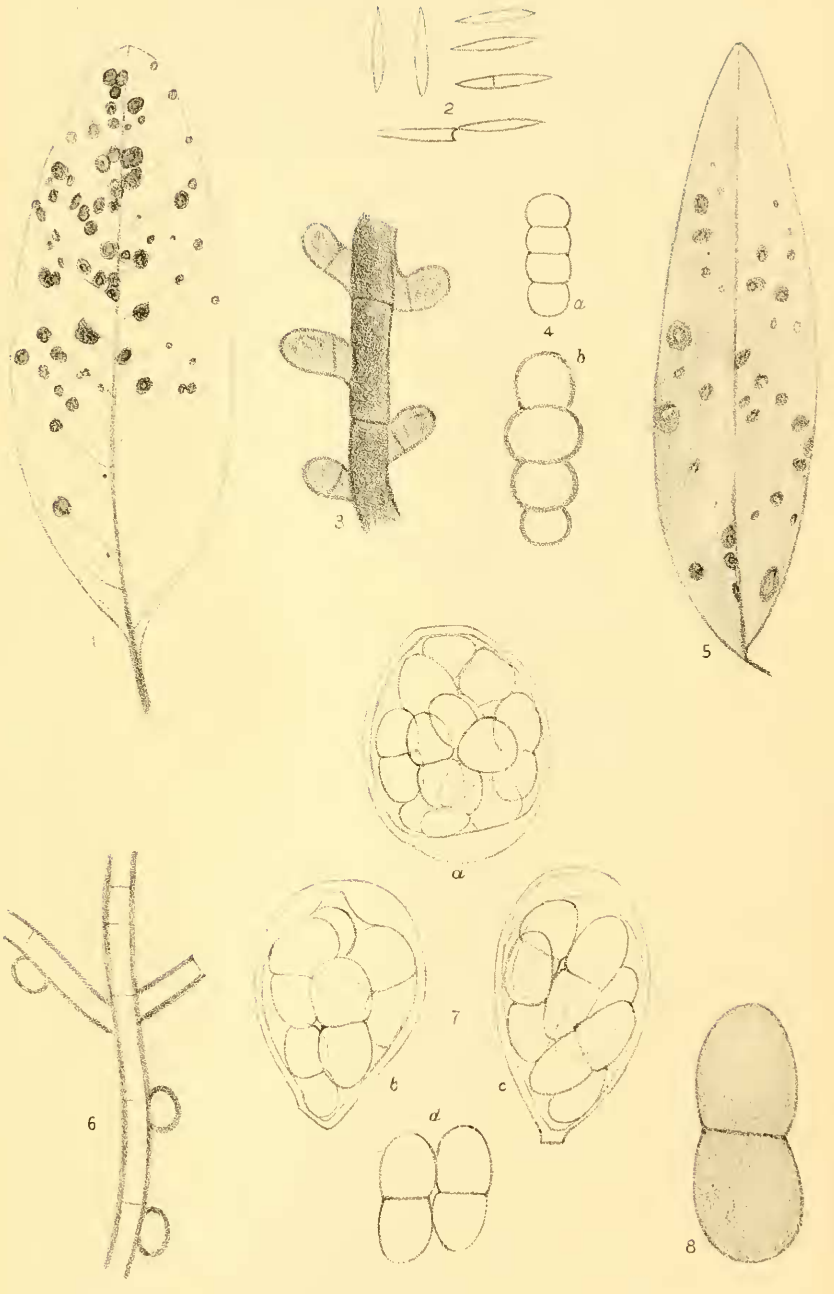

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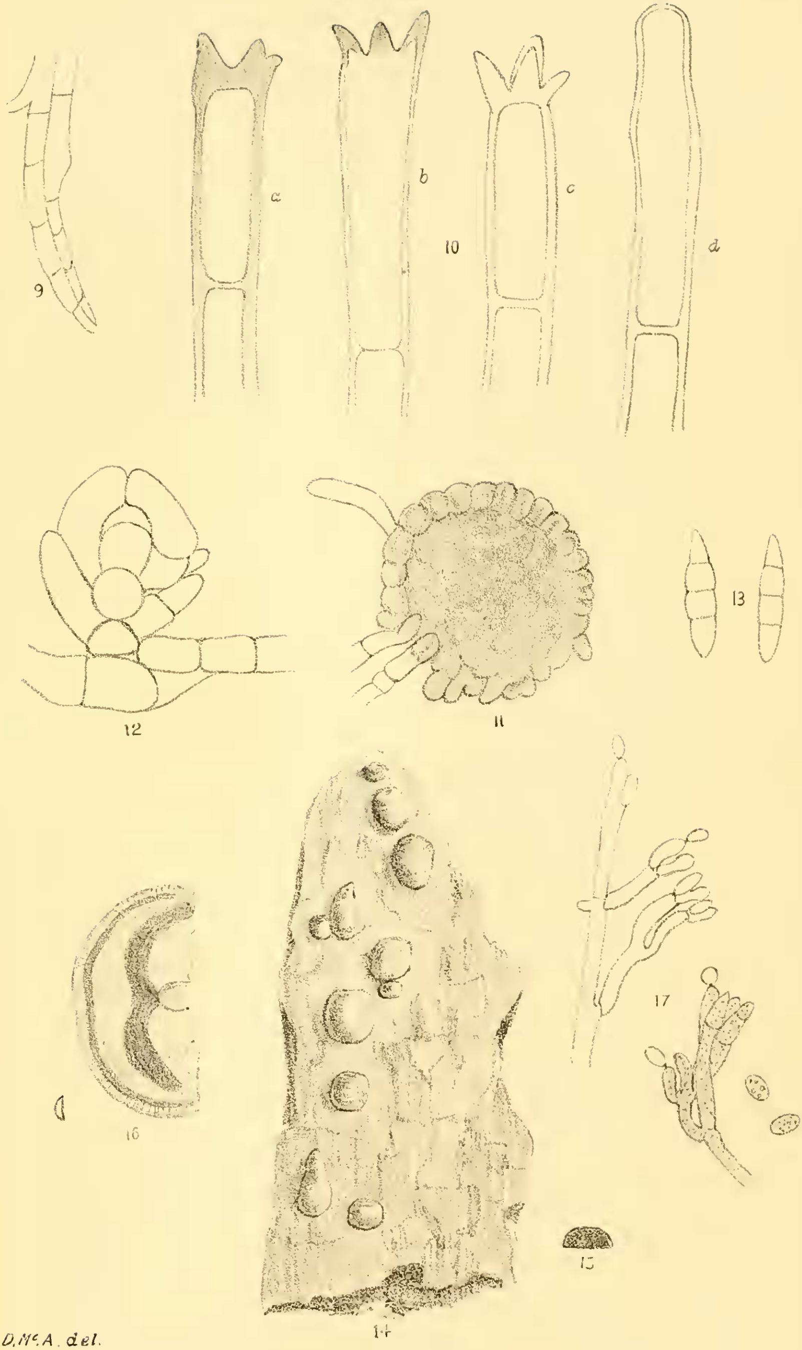

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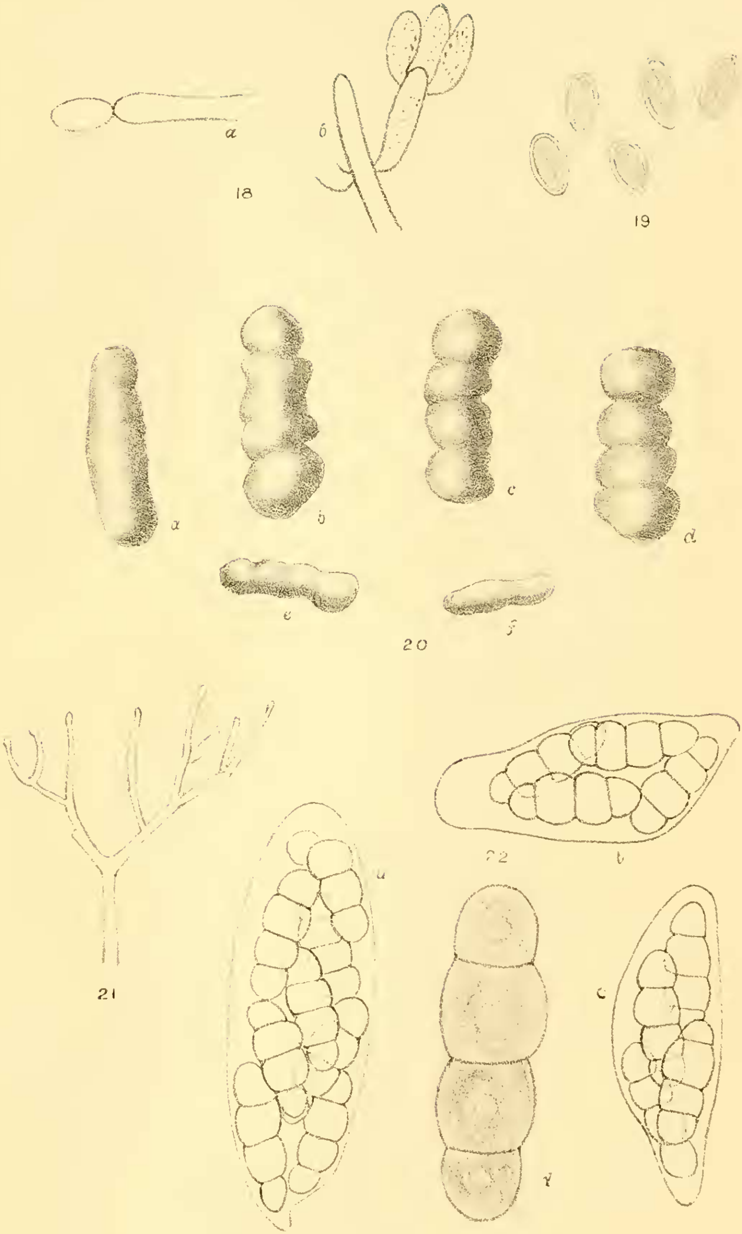

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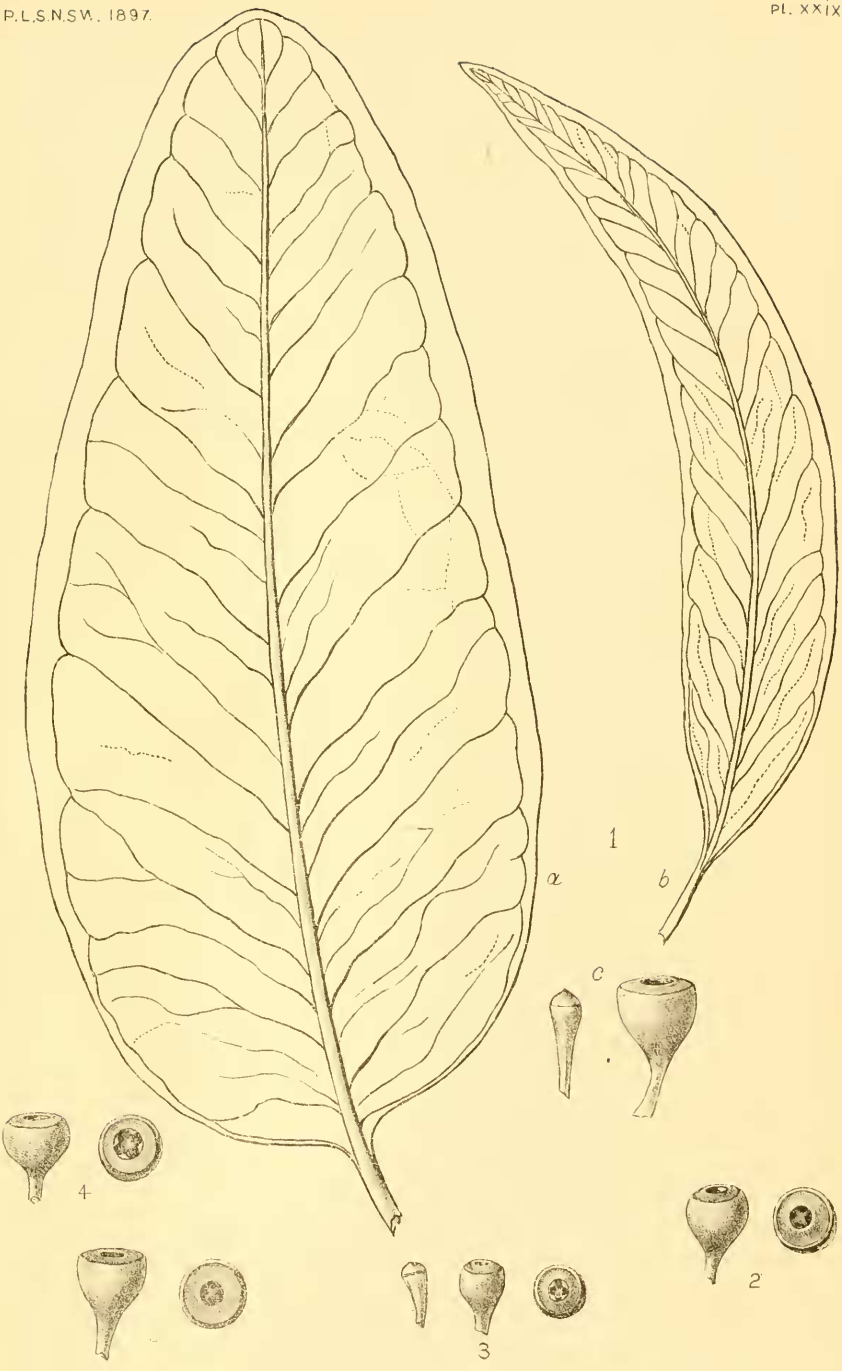

E HÆMASTOMA. Sm

E. HEMASTOMA (MICRANTHA).

HÆMASTOMA (MICRANTHA) *Figs.* 8-14. E. STRICTA *Figs.* 15-17.